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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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The Journal *of* Parapsychology

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This JOURNAL, published on the thirtieth day of March, June, September, and December, is devoted primarily to the original publication of experimental results and other research findings. In addition, articles presenting reviews of the literature relevant to parapsychology, criticisms of published work, proposals for research, theoretical and philosophical discussions, reports of spontaneous cases, and new methods of mathematical analysis will be published as JOURNAL space allows. The editors welcome comments, inquiries, and suggestions about the JOURNAL and any of its contents.

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RESEARCH ON TELEPATHY IN SOVIET RUSSIA

By MILAN RÝZL[1]

RESEARCH on parapsychical phenomena in the U.S.S.R.[2] has taken another direction than we have been accustomed to see in English-speaking countries. With regard to the Western world we are not far from the mark when we say that in Western countries it is primarily the psychologists who have concerned themselves with research on these phenomena, and that therefore the methods used have mainly been psychological in character. In the U.S.S.R. parapsychological phenomena have chiefly aroused the interest of physiologists, and this interest has consequently been reflected in the approach to these problems. In fact, physiological methods have been used predominantly.

There are, however, factors other than the difference in methods

[1] Dr. Ryzl, of the research staff of the Institute of Biology in Prague, has already contributed to this *Journal* as well as to other periodicals in his own country and elsewhere. A sketch of his parapsychological career was given in the *Parapsychology Bulletin* of May, 1960. He has been active in ESP research for several years and through his knowledge of languages is in relatively close touch with other workers in this field in Eastern Europe. For this reason he was in a good position for preparing this review. All of the references given in English are his translations of Russian titles and sources.—Ed.

[2] This paper deals only with the research work in parapsychology in Soviet Russia (during the Soviet era), and with studies directly related to this research work. This paper does not claim to be an exhaustive study of parapsychological research in Russia. Earlier observations of physical mediumship, for instance, are not mentioned at all, and experiments on telepathy from earlier times (Nom Kotik, etc.) have not been included. Since attention is focused mainly on later studies, even Bekhterev's important experiments have not been given such detailed attention as their significance deserves.

used that are worth mentioning. In the U.S.S.R. the development of science is regarded as a matter of nation-wide interest, and considerable financial means are being made available for the benefit of science and research work. Parapsychology, however, does not yet belong among acknowledged branches of science supported in this way, and the research on psi phenomena has been considered, at most, a private hobby of some individuals. Even though the interest in the research on psi phenomena in the U.S.S.R. still exists, the research has for long decades not been organized properly. Only a few individual followers of Academician Bekhterev have contributed to the increase of experimental knowledge. Soon after World War I, Bekhterev published the results of his interesting telepathy experiments with dogs,[3] and we shall have the opportunity of dealing with them later on.

The hostile attitude of the scientific public who saw in parapsychology only a variety of occultism has prevented, in the long run, the developing of the research to its full extent. The attention of the Soviet scientists was confined to the research in telepathy, which seemed to be the simplest of all psi phenomena, and also seemed to be the easiest to accept (the more so since the hypothesis of signals on the basis of electromagnetic waves was available as an explanation). The research in telepathy could thus become a basis on which the edifice of research on all psi phenomena could be erected.

Nevertheless it happens that the results of experiments carried on, say, as far back as in the thirties of this century are only now being published.

But let us cast a longer look back into history. We find there that as early as the second half of the nineteenth century, the well-known Russian physiologist V. Ya. Danilevskyi showed an interest in the research in telepathy. In the beginning of the twenties of this century the Academicians V. M. Bekhterev, P. P. Lazarev, and A. V. Leontovitch carried on experiments in Moscow with the transfer of thoughts from man to dog.

A large number of experiments of this kind have been carried out. Let us consider experiments in which an excellent subject was

[3] Bekhterev, V. M. Concerning the experiments on "thought" influence on the behavior of animals. *Questions of Personality Study and Education*, No. 2 (1920).

found in the dog Mars, presented by the animal-tamer V. L. Durov. These experiments were carried out to investigate whether or not the dog would receive and carry out an order given him by Durov only in thoughts. In a particularly successful experiment, Durov was to give the dog an order to bark several times in succession. The number was to be selected by the experimenters, but Durov told them that the chosen number should not surpass seven, since otherwise there would be the danger of the dog's getting accustomed to barking and of his continuing to bark automatically regardless of the number.

The experiment took the following course: Durov and the other experimenters remained in one room, and one of the observers, Academician Leontovitch, took the dog to another, distant room. This was to ensure that Durov would be unable to give the dog either a visual or an auditory signal. Then Bekhterev handed Durov a slip of paper with the required number of barks, a number which was not known to any of the participants (it was 14). Durov looked at the slip, was surprised at the unexpected number, wrote something on the slip, and proceeded to carry out the experiment.

Academician Leontovitch gave on his return this report of the happenings in the other room: On arriving in the room, the dog lay down on the floor. Soon afterwards he drew himself up on his forelegs, cocked his ears as in attention, gave seven barks, and lay down again. Leontovitch was thinking that the experiment was over and wanted to take the dog back, when the dog again sat up, cocked his ears, again barked seven times, and again lay down. The explanation was that Durov, in order not to exceed the upper limit of seven barks, decided to solve the task by dividing the 14 barks required into two independent tasks of seven barks each; and put it down on Bekhterev's slip as $7 + 7$.

Another, more complicated, experiment was carried out: The dog was to be given a telepathic order to find an object unknown to him and placed in a different room where no experiments had been carried out with him before, and where he would be left alone while choosing the object. To render the experiment more difficult, an object was chosen which was not within the range of vision of the animal when in its normal position. The task was set in the following way: In a small room outside the laboratory there were three

tables, one higher than the others, with a telephone and several books on it. The dog was to approach that table, to select the telephone directory from among the books lying on it, to take the directory and carry it to the laboratory.

Durov's procedure was this: He took the dog's head in his palms and stared motionlessly into his eyes. The dog being unable to carry out the command given in thought, the concentration was repeated a second time, and finally a third time. Only after the third repetition did the dog run through the open door into the small room, stand up on his hind legs at one of the tables, but finding nothing there, let himself down to the floor again. Then he went over to the second table and rose again; but in spite of the fact that there were several objects lying about, he did not take anything and again let himself down to the floor. Only on arriving at the table with the telephone and raising himself up the third time did he take the telephone directory between his teeth and bring it into the laboratory. The correct fulfillment of the task led to the conclusion that an idea of the telephone directory must have arisen in the dog's mind as it was emitted by the human being. The experiment testifies to the conclusion that under certain conditions the direct transmission of thoughts from man to animal is possible.

There is no denying the fact that the results of these experiments are surprising in the exact fulfillment of the telepathic order. Nevertheless, they suffer from the same shortcomings of all qualitative experiments. There is no exact statistical evaluation; and, therefore, the greatest exactness in the fulfillment carries no sufficient conviction in the light of the methods of evaluation introduced today in parapsychology. This is why the experiments fall short of any exact scientific requirements; and this is certainly the reason why they failed to meet even in the U.S.S.R. with a greater response, despite Bekhterev's great scientific authority.

The Academicians Bekhterev and Lazarev then concluded that the most acceptable explanation of these observations was the assumption that electromagnetic waves were traveling between the agent and the percipient. In 1922, Lazarev, in his book *Physico-Chemical Principles of Higher Nervous Activity* (p. 46), expressed the opinion that electrical action currents accompanying physical-

chemical reactions in the neurons of the cortex must generate electromagnetic waves which radiate from the head of the human being and can serve as an explanation of telepathy. Lazarev concluded that this role can be played particularly by waves of great length (about 30,000 km).

Bekhterev's experiments with Durov and Mars were later continued by B. B. Kazhinskyi, who, in his book *Thought Transference* (Moscow, 1923), supplied further arguments in favor of the electromagnetic theory of the explanation of telepathy. Kazhinskyi, for example, carried out an experiment in which Durov was shut up in a Faraday cage which isolated him against electromagnetic waves. In these conditions it was quite impossible to enforce any order upon Mars. When the door was opened, however, Mars began precisely to fulfill the orders given him mentally.

A similar conclusion was possible also from experiments carried out by L. Vodolazskyi and T. Gursteyn.[4] They used a subject in the hypnotic state who was in an electromagnetically isolated chamber together with the observers who were registering the subject's behavior. The hypnotist, with another observer, were in another room near by. At an agreed moment the experiment began: The hypnotist "sent" to the subject successive impulses inducing him to execute various movements (e.g., to raise the right hand, both hands, etc.). The impulses were recorded, and so were the movements of the subject. Both records were then checked.

In the course of the experiment it was possible to interrupt or restore the screening capacity of the chamber by opening or shutting its door. These changes were made without the hypnotist or his companion having any knowledge of them. The comparison of the records revealed that with the door open the subject complied with the orders relatively easily, but with the door shut the telepathic orders were not fulfilled even once correctly.

In 1942, the Soviet biologist Prof. S. Ya Turlygin[5] published a work in which he described his telepathic experiments carried out with the assistance of the then famous hypnotist Ornaldo.

[4] Brain apparently exhibits electromagnetic activity. *Technics for Youth*, No. 2 (1961). P. 33.

[5] See Mirza, D. Do not forget chance coincidences. *Technics for Youth*, No. 2 (1961). P. 30.

The hypnotist was put into a chamber with lead screening. In front of his face there was a hole in the wall which could be covered with dark paper so as to prevent the hypnotist from seeing through it. The hole could also be covered with a lead partition which could be moved noiselessly according to the experimenter's choice. The subject was placed in a second room. Neither subject nor hypnotist could see whether the hole was open or screened off by lead.

The subject had been conditioned to fall backward on receiving a telepathic impulse. (This reaction was chosen because it is often used also in other experiments with hypnosis.) In the experiment proper the hypnotist tried to transmit this impulse telepathically to the subject. When the subject received it, he felt inclined to fall backward, lost his balance, and was caught by the assistants standing by. The results of this series showed that the lapse of time between the emission of the light signal to the hypnotist asking him to start to "transmit" the telepathic impulse "to fall back" and the fall of the subject was always substantially longer when the hole was closed than when it was open. If, under the same conditions, a metal mirror was placed in front of the hole, the effect occurred when the subject was in a position to which the assumed waves would be deflected by the mirror.

All these experiments really seemed to be a very strong argument for the electromagnetic nature of telepathy, but yet they were not final proof because it was not out of the question that the failure of the telepathic transfer was caused by another unknown factor acting concomitantly with the screening-off of the chamber.

The explanation of telepathy by electromagnetic undulation has always entailed some considerable difficulty. It is true that the electromagnetic undulation spreads through space quite imperceptibly to our senses, and if picked up by an appropriate receiver it may serve as a means to transfer intelligent news. Of course, so far, it has been only a model of a telepathic process. In order that this model may become an explanation, it must first be proved that the agent's brain is really able to transmit electromagnetic waves into the surrounding space and that the waves thus emitted can really be caught by the percipient.

In this phase of explanation we meet already with complications.

It is true that all processes in the brain are accompanied by electric action currents. When these currents are adequately amplified, we can register them by the electroencephalograph. The electroencephalographic record is then able to give us many items of information on processes occurring in the brain. It could then be assumed that these cerebral currents were the generator of the expanding electromagnetic waves able to stimulate the respective receptors. As a matter of fact, no reception organ which could be made responsible for the catching of such signals has been found in the brain by anatomical study. This, however, as was correctly pointed out by the Soviet physiologist Saradzhev,[6] need not be a principal obstacle, for the organ of reception specializing in catching telepathic signals may well be imagined to be placed in the skin, for example, where various microscopic formations have been ascertained whose functions in the organism are still unknown. The trouble is, however, that the cerebral currents just spoken of are infinitely weak. It can hardly be imagined that they could produce a perceptible effect at a distance greater than a few centimeters. A telepathic transfer, however, has been observed even at a distance of thousands of kilometers.

But the main difficulty of an electromagnetic explanation of telepathy is still elsewhere. The electroencephalographic record registers the total of electric activity of millions of nerve cells whose electric effects are summed up. The electric activity which we are able to catch outside the brain cannot give us an account of the activity of every individual neuron. For this reason the electroencephalogram can inform us only of actions in which vast groups of neurons participate. It can distinguish whether the subject is sleeping or awake; it spots disorders in the brain tissue; but it cannot help us recognize what the subject is thinking about at a given moment. The contents of thoughts are not recognizable in this way—but this is the chief supposition for a successful transfer of thoughts.

The proper experimental test of the electromagnetic hypothesis was carried on along two principal lines:

First was the investigation of whether the human brain emits electromagnetic waves. Here the Soviet physiologists used as

[6] Saradzhev, N. Is the brain able to perceive thought transference directly? *Technics for Youth*, No. 1 (1961). P. 31.

the basis of their work the observations of the Italian neurophysiologist F. Cazzamalli[7] who in the twenties of this century carried on experiments in which he tried to catch the supposed cerebral radiations by means of an aerial. Cazzamalli took it for granted that he was really successful in catching such waves, and concluded that the basis of telepathy could be electromagnetic waves of a relatively short wave length (about one meter).

Cazzamalli's experiments, however, have been criticized several times. One of the chief objections was that there was no adequate screening-off of disturbing radiations coming from the surroundings. There was no guarantee that the waves caught by Cazzamalli really came from the subject's brain. In the Leningrad Institute of Brain Physiology, Cazzamalli's experiments were repeated without achieving positive results.[8]

The first question was, therefore, not definitely answered. There is, however, still another side to the problem. Even if the transmitter's brain were capable of transmitting electromagnetic waves, there remains the question of catching them by the recipient's organism. In studying this problem, the various authors endeavored to examine the effect of electromagnetic fields on the nervous system. As far back as 1900 the works of the well-known Russian physiologist V. Ya. Danilevskyi[9] showed that it was possible to stimulate isolated nerves as well as muscular fibers by electromagnetic fields of low frequencies (50-100 cps), as well as to increase the irritability of the central nervous system of animals and human beings. Similar results have also been attained in studying the effect of electromagnetic fields of high frequencies, even though the mechanism of their influence on living tissue is different from that of fields of low frequencies. Thus, for instance, P. P. Lazarev's successor, S. Ya.

[7] Cazzamalli, F. Les ondes électro-magnétiques en corrélation avec certains phénomènes psycho-sensoriels. *Comptes Rendus* du III-e Congrès International des Recherches Psychiques, Paris, 1928.

[8] See Vasiliev, L. L. *Mysterious Phenomena of the Human Psyche* (1959). P. 102. For critics of Cazzamalli's experiments, see also: Petrovskyi, A. A. Telepsychic phenomena and brain radiations. *Wireless Telegraphy and Telephony*, No. 34 (1926). P. 61.

[9] *Researches on the Influence of Electricity at a Distance.* Vol. I, (1900); Vol. II (1901).

Turlygin,[10] found that the influence of about one-inch electromagnetic waves increases the sensitivity of the nervous system.

Very remarkable also was the result of the experiment made at I. P. Pavlov's Institute of Physiology, in which a conditioned reflex could be attained in subjects on the otherwise not perceivable low frequency (50 cps) electromagnetic field.[11]

We may then take it for granted that the electromagnetic waves are able to influence the brain cortex. The mechanism of the effect is not quite clear. With fields of low frequencies we can imagine that it is the question of electro-induction. We must, of course, realize that the electromagnetic waves can act upon the brain only if their intensity attains a certain minimum level value. This question has been analyzed by the Soviet physicist V. Arkadev.[12] He has come to the conclusion that the intensity of electromagnetic fields which can be created by electric currents in the living brain is too small, and cannot attain the necessary minimum level quantities. As a matter of fact, it is much smaller than the intensities of electromagnetic fields created by conductors of electricity in the vicinity of which man constantly lives. On the basis of these two considerations Arkadev considers the electromagnetic hypothesis incorrect.

The definite answer seems to follow from experiments carried on by the Soviet physiologist, Bekhterev's pupil, Prof. L. L. Vasiliev.[13] In 1932 he found three very susceptible persons with whom he carried on experiments in which the subject was hypnotized at a distance by a mere telepathic impulse.

In these experiments, the subject was placed in a particular room;

[10] Turlygin, S. Ya. Influence of centimeter waves on the central nervous system. *Proc. Academy of Science, U.S.S.R.* Vol. XVII, No. 1-2 (1937). P. 19.
New data about this theme may be found also in *Biological Effects of the High-frequency Electromagnetic Field* (A. V. Triumfov, Ed.), 1957.
For some theoretical considerations see also the article by A. V. Leontovitch in the *Anniversary Volume of the Ukrainian Academy of Science.* Vol. I., 1944.

[11] Petrov, F. P. Influence of low-frequency electromagnetic fields on higher nervous activity. *Proc. of I. P. Pavlov's Institute of Physiology.* Vol. I. (1952). P. 369.

[12] About the electromagnetic theory of the transfer of mental suggestion. *J. Applied Physics.* Vol. I (1924). P. 215.

[13] Personal communication. Results are about to be published in a special monograph by Vasiliev. For a short account see the article by Gleb Anfilov: Encountering the incomprehensible. *Knowledge—Power,* No. 12 (1960). Pp. 19-20. (A translation of Anfilov's article is scheduled for early publication in the June issue of *The Soviet Review,* 156 Fifth Ave., New York 10, N. Y.—Ed.)

in the adjoining room was the controller watching the recording instruments. In a distant room (separated by two intervening walls) was the hypnotist. The observer at the registering instruments did not know the program of the experiments (this was known only to the hypnotist).

The experiment proper had the following course: On the basis of a random process (by means of a roulette), the moment was chosen when the hypnotist sent the hypnotizing impulse to the subject. The latter was brought into a hypnotic state. The process of falling asleep was automatically registered by the recording instruments: on the one hand by measuring the electric potentials on the skin, and on the other by a mechanical pencil which was put into action by the subject as he regularly pressed a rubber bulb (every pressure being recorded as a tooth-like notch on the graph). As soon as the subject was hypnotized he stopped pressing the bulb, and the recording showed a straight line. (Sometimes the subject resisted hypnosis, and this struggle against sleep also showed itself in the character of the recording.)

The hypnotized subject was awakened after a certain time. Often the corresponding telepathic impulse was sufficient to wake up the subject. Remarkable, however, were the instances when the emitted telepathic impulse was not sufficient for a complete awakening. In this case the recording machine recorded at least "the endeavor of the subject to awake." The subject pressed the bulb several times, which resulted in several notches in the line; but later on the impulse died away, and the subject went on to sleep.

In this telepathic hypnosis, the most conspicuous results were obtained in the first experiments. Later on, autohypnosis began to assert itself in a disturbing manner. After several series of experiments, the subject developed a conditioned sleep reflex and went to sleep even without a telepathic impulse as soon as he was brought into the usual conditions. But even in this complicated situation telepathy manifested itself clearly. When the telepathic impulse was transmitted, hypnosis was attained two to three times faster than in cases where the impulse was not transmitted.

These experiments were developed further. Thus, for example, the selectivity of the telepathic factor became apparent. If all the

three subjects were placed together, the hypnotist always succeeded in hypnotizing the one he had selected beforehand. Besides, the hypnotized subject was always aware of who it was who hypnotized him and was able, when asked to do so, to name correctly which one of three hypnotists had given the telepathic hypnotizing impulse.

When the manifestation of telepathy was mastered, new experiments could be conducted to examine the correctness of the telepathic explanation. The hypnotist was shut up in a cabin of thick lead slabs. The roof of this cabin was made in such a manner that its walls, when the cabin was closed, could be put into grooves filled with mercury. Every precaution was taken that no electromagnetic waves of any length could penetrate from the inner cabin to the outside. And yet, all results of experiments with the telepathic hypnosis were, even under these conditions, *absolutely the same as before.* The subject went to sleep again in accordance with the telepathic impulses, the auto-hypnosis could be equally hastened, and experiments with selective hypnotizing succeeded as well as before.

These experiments of Vasiliev's, as well as the present organizing activity of Prof. Vasiliev, offer significant promise of further development of the research in parapsychology in the U.S.S.R.

At the present time, the interest in parapsychological problems in the U.S.S.R. (as well as in other East European countries, of which Czechoslovakia, Poland, East Germany, and Yugoslavia deserve to be mentioned) is rapidly increasing. Some research work is going on in Moscow, but the chief work is being conducted now by Prof. Vasiliev in the Physiological Institute of the Leningrad University. Especially concerned with this research is the Laboratory of Physiological Cybernetics of this institute headed by Vasiliev's pupil, Dr. P. I. Gulyaev.

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A CONTROVERSY OVER CHARGES OF FRAUD IN ESP

EDITORIAL INTRODUCTION

THE FOLLOWING four articles constitute a controversy which has recently arisen concerning the possibility of fraud in two ESP experiments of the thirties. The critic who makes the accusation is Mr. C. E. M. Hansel, Lecturer in Psychology at the University of Manchester in England, and the experiments against which he makes this charge are known as the Pearce-Pratt series, first published in 1937, and the Pratt-Woodruff series, published in 1939. The experimenters who were involved in those researches were Dr. J. B. Rhine, Director of the Parapsychology Laboratory of Duke University; Dr. J. G. Pratt, Assistant Director and one of the editors of this *Journal;* and Dr. J. L. Woodruff, Associate Professor of Psychology, City College of New York, and Secretary and Member of the Board of Trustees of the American Society for Psychical Research.

It has always been the editorial policy of the *Journal of Parapsychology* to welcome critical articles, although the editors have preferred, when possible, to clear up disagreements out of court, so to speak. This could have been done in the present instance if Mr. Hansel had so desired, for he was at the Parapsychology Laboratory for a number of weeks, having been invited there to facilitate his examination of the literature of the field. But he preferred to submit his papers for publication after his departure, thus precluding any other means of carrying on the discussion. On this account, his charges are being published here in full, together with the replies of Drs. Rhine, Pratt, and Woodruff.

A CRITICAL ANALYSIS OF THE PEARCE-PRATT EXPERIMENT

By C. E. M. HANSEL

In the Pearce-Pratt experiment[1] the subject obtained high scores when guessing cards whose identities were unknown to any other person.

The subject was a student named Hubert Pearce. He obtained high scores under two conditions:

(A) When the cards were in a room on the top floor of the Social Science Building of Duke West Campus, approximately 100 yards away from him;

(B) When the cards were in a room on the top floor of the Medical Building approximately 250 yards away from him.

PROCEDURE

The general procedure adopted in the experiments is described as follows in the report:—

> At the time agreed upon, H.E.P. visited J.G.P. in his research room on the top floor of what is now the Social Science Building on the main Duke campus. The two men synchronized their watches and set an exact time for starting the test, allowing enough time for H.E.P. to cross the quadrangle to the Duke Library where he occupied a cubicle in the stacks at the back of the building. From his window J.G.P. could see H.E.P. enter the Library.
>
> J.G.P. then selected a pack of ESP cards from several packs always available in the room. He gave this pack of cards a number of dovetail shuffles and a final cut, keeping them face-down throughout. He then placed the pack on the right-hand side of the table at which he was sitting. In the center of the table was a closed book on which it had been agreed with H.E.P. that the card for each trial would be placed. At the minute set for starting the test, J.G.P. lifted the top card from the inverted deck, placed it face-down on the book, and allowed it to remain there for approximately a full minute. At the beginning of the next minute this card was picked up with the left hand and laid, still face-down, on the left-hand side of the table, while with the right hand J.G.P. picked up the next card and put it on the

[1] Rhine, J. B., and Pratt, J. G. A review of the Pearce-Pratt distance series of ESP tests. *J. Parapsychol.*, 1954, **18**, 165-78.

book. At the end of the second minute, this card was placed on top of the one on the left and the next one was put on the book. In this way, at the rate of one card per minute, the entire pack of 25 cards went through the process of being isolated, one card at a time, on the book in the center of the table, where it was the target or stimulus object for that ESP trial.

In his cubicle in the Library, H.E.P. attempted to identify the target cards, minute by minute, and recorded his responses in pencil. At the end of the run, there was on most test days a rest period of five minutes before a second run followed in exactly the same way. H.E.P. made a duplicate of his call record, signed one copy, and sealed it in an envelope for J.B.R. Over in his room J.G.P. recorded the card order for the two decks used in the test as soon as the second run was finished. This record, too, was in duplicate, one copy of which was signed and sealed in an envelope for J.B.R. The two sealed records were delivered personally to J.B.R., most of the time before J.G.P. and H.E.P. compared their records and scored the number of successes. On the few occasions when J.G.P. and H.E.P. met and compared their unsealed duplicates before both of them had delivered their sealed records to J.B.R., the data could not have been changed without collusion, as J.G.P. kept the results from the unsealed records and any discrepancy between them and J.B.R.'s results would have been noticed. In Subseries D, J.B.R. was on hand to receive the duplicates as the two other men met immediately after each session for the checkup.

Uncontrolled Features of the Experiment

The results of the four subseries were all such as to make it certain that, in the absence of ESP, there must have been some premeditated plan to obtain high scores by normal means. If H.E.P. had wished to carry out a trick, it is clear that he had every opportunity to do so. From the time he entered the library, he was unobserved. He had a complete time schedule so that he knew to the minute the stage which the experiment had reached, and he also knew that during this time J.G.P. was occupied with the cards. He had 50 minutes to walk back to the Social Science Building or Medical Building to obtain sight of the cards. Thus we have to investigate the possibility of his being able to obtain information in this way. For this purpose it is necessary to know in detail the arrangements in each of the rooms used by J.G.P. We also note that the procedure adopted by J.G.P. would have made it virtually impossible for any sight to have been got of the cards except when he was

recording them at the end of the experiment or when he left the room after the experiment.

While at the Parapsychology Laboratory I asked Dr. Pratt to demonstrate the procedure he adopted, and it was clear that the cards were made clearly visible during the recording of the targets at the end of the runs. I also ascertained that he did not lock his door, that the packs of cards were left unshuffled after the experiment, and that he made his record on notepaper. Dr. Pratt told me that he took his copy of the targets to Dr. Rhine but that he did not see H.E.P. until some time (unspecified) after each sitting. The following possibilities therefore present themselves at this point.

(1) That H.E.P. in some way got sight of the cards as they were turned up for recording.

(2) That H.E.P. waited until J.G.P. left the room after the experiment to take his copy of the target record to Dr. Rhine, and then entered the room and recorded the targets from the packs.

(3) That H.E.P. saw the second copy of the target lists, assuming that J.G.P. had left this on his desk, or obtained an imprint of

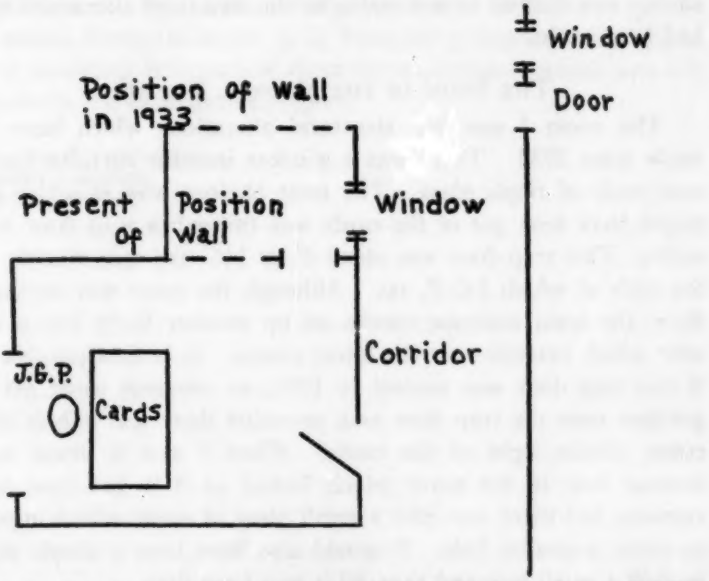

FIG. 1. Plan (not to scale) of the room (314) in the Social Science Building.

it from the next sheet on a pad of notepaper on which J.G.P. made his record.

(4) That H.E.P. had an assistant who obtained a record of the cards in one of the above ways.

The Room in the Social Science Building

In this room Dr. Pratt sat with his back to the window (Fig. 1). There was a pane of clear glass above the door, but it would have been a difficult matter for a short person like H.E.P. to stand on a chair and look through. I understood from J.G.P. that the wall on one side of the room had been moved since the experiment. In its previous position a clear glass window about 2′ square was available for anyone to look through from the corridor. The bottom of this window was about 5′ 10″ from the floor, and only a tall man in the corridor could conceivably have seen through it without standing on a chair. There is, however, a clear pane of glass above the door in a room on the opposite side further down the corridor through which sight might have been got of the cards by anyone standing on a chair inside the room with the door shut. This possibility was difficult to test owing to the structural alterations which had been made.

The Room in the Medical Building

The room I saw has structural alterations which have been made since 1933. There was a window into the corridor but this was made of ripple glass. The most obvious way in which sight might have been got of the cards was through a trap door in the ceiling. This trap door was about 4′ by 1½′ and was directly over the table at which J.G.P. sat. Although the room was on the top floor, the main staircase carries on up another flight into a large attic which extends over the lower rooms. It is thus possible that if this trap door was present in 1933, an observer could get into position over the trap door and, provided there was a hole in the cover, obtain sight of the cards. When I saw it, there was a circular hole in the cover which looked as if it had been added recently, but there was also a small piece of metal which appeared to cover a smaller hole. It would also have been a simple matter to drill a small hole and then fill it at a later date.

When the experiments were transferred to the room in the Medical Building, the scores dropped for the first five sittings to the chance level. If a trick was employed, this might have been due to difficulty in getting the relevant information under the new conditions. Scores also sometimes dropped for a complete sitting. Thus they were well above chance for the two sittings in which runs 37, 38, and runs 43, 44 were completed, but they were below the chance level for the two sittings in which runs 39, 40 and 41, 42 were completed. This effect might have arisen if on some days there was difficulty in getting into a position where sight of the cards or the records could be obtained.

Thus as the experimental conditions were such that above-chance scores could have been obtained by means of a trick, this experiment cannot be cited as providing evidence for ESP.

I also understand that statements have been made that H.E.P. was "seen somewhere where he should not have been." If by this is meant that he was seen wandering around when he should have been in the cubicle, or if he was seen leaving either of the rooms used by J.G.P. after the experiments, such statements should be brought into the light and carefully examined.

I would like to thank Dr. J. G. Pratt for giving me every assistance in obtaining information about the experiments which was not available in the experimental report.

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A REPLY TO THE HANSEL CRITIQUE OF THE PEARCE-PRATT SERIES

By J. B. RHINE AND J. G. PRATT

In spite of his unconcealed eagerness to give the *coup de grâce* to a piece of ESP research, Mr. Hansel is entitled to our appreciation for bringing our experiment of twenty-seven years ago momentarily back into the limelight. This acknowledgment may be linked with the information that, although Hansel's negative approach to parapsychology was a matter of public knowledge, he was invited and given a travel grant by the Duke Laboratory to make the visit which resulted in his paper. There are still other points of value arising from his critique, but they can best be left to follow our evaluation of his remarks about the experiment.

We cannot, of course, condone Hansel's methods; instead of directly endeavoring to clear up his differences with the authors (which might have needed a few minutes' discussion when he was here), he has gone off into print without asking the reaction of the authors. Whether he really has the exceedingly strong case this course of action might suggest or is only exceedingly motivated to damage the evidence for ESP on the grounds of his own prejudgment, the reader will be able to determine for himself.

Comment might first be made on the technique of Hansel's climactic revelation that he has heard a rumor that H.E.P. was "seen somewhere where he should not have been." A rumor is hardly a proper basis for a scientific criticism; but even if such a rumor had any support (and we know of none), our comment would still be that if the conditions of the experiment were adequate (as we shall show they were), it should have been impossible for the rumored circumstance to have produced the results. In other words, the proper focus is so obviously on the adequacy of the methods that the introduction of the rumor is not in order.

ADEQUACY OF TEST CONDITIONS

Were the test conditions adequate? On this issue, there is one very essential point to get straight at the outset, and Hansel has

not got it straight. Any piece of research under criticism obviously stands or falls on its strongest, best-controlled section. Anyone confining his attack to any other section is either misled or misleading or both. And that is precisely what Hansel does. He avoids mention of the most advanced section of the experiment, Series D, and confines his attention entirely to the section comprising Series A, B, and C. He is attacking a non-vital organ.

We ourselves, of course, did not stop with Series A, B, and C. If there were any reason to pause and analyze Hansel's remarks about the first three series in terms of the actual situation, it could be shown that, at that time and stage, the procedure represented a definite advance over previous work. The strained alternative hypotheses Hansel suggests are unrealistic to those acquainted with the situation. H.E.P. had no knowledge he was not being trailed, and any spying or collusion on his part would have been most obvious in a department (of psychology) in which there were skeptics ready to suspect him of trickery. The devices proposed by Hansel would have been conspicuous and clumsy in crowded corridors. But for general reasons of precaution, it was recognized at the time that the conditions were such that the validity of the results depended entirely on the experimenter, J.G.P. In order to broaden the base of responsibility and protect both the experimenter and the results from possible charges of fraud, such as those now leveled, the conditions were strengthened in the last sub-series of the experiment reported. It is to this improved stage, Section D, that attention must be given for a proper judgment.

We are now ready to look at that series. As a matter of fact, it is not easily overlooked and would be, for most readers, quite obviously the climax series in the paper. First of all, it is in its own right statistically significant, and its scoring average is above that of the paper as a whole. It can well bear the burden of the conclusion by itself. The next question, then, is how Hansel's criticisms apply to it and its conditions.

In this series, J.B.R., who had remained in the background previously, came into the test room with J.G.P. and sat through a series of six runs through the test pack (150 trials) for the purpose of scrutinizing the entire procedure from that point of vantage, to ensure that it was faithfully executed. He, like J.G.P., *could see*

the subject from the window as the latter entered the library (and, of course, could see him exit as well). He was in the experimental room at the end of each session to receive the independent records from both J.G.P. and H.E.P. immediately on the arrival of the latter at the close of the session. Thus the subject was obviously allowed no opportunity to enter the room alone and copy the order of the cards or the impressions left on the record pad. Even with the somewhat imaginative supposition that H.E.P. had a collaborator, there was no time for the latter, even if he had (unnoticed by J.B.R.) observed the card-turning and recording by J.G.P., to have communicated the knowledge of card order thus gained to H.E.P. as he arrived in the building for the check-up. H.E.P. had to have his duplicate record in his own handwriting, with one copy sealed in an envelope, ready to hand to J.B.R. on entering the room. J.G.P. had to do the recording of the last run of each session after the test was over and H.E.P. was already on his way to the test room. Yet these final runs of the session were, in themselves, independently significant statistically.

It is clear, then, not only that Hansel's counterhypothesis does not apply to Series D, but also that he did not intend it to do so. One can only wonder why he did not deal with this series and, if he is interested in the case for psi, why he did not follow on up the trail of methodological advances into other researches in parapsychology over the intervening decades. He would have found that the question of honesty which (in this and other papers) preoccupies him with such peculiar personal absorption was practically banished as the research entered the investigation of precognition, in which, of course, the target series is not in existence for some time after the subject has committed his responses to paper.

But the motives of the critic are only of secondary importance here; of much more concern to us is the fact that many students of the field are properly seeking an objective evaluation of the case for parapsychological abilities. The body of fact in parapsychology is like a many-celled organism. Its strength is that of a growth-relationship, consisting not only of the compounding of one cell with another, but also of the many lawful interrelations that emerge in the growing structure. Going back as Hansel has done, with a one-cell perspective, to fix attention on some incomplete stage of

development within a single experimental research is hard to understand in terms of healthy scientific motivation.

Positive Values of the Critique

So much for the lack of intrinsic worth in Hansel's paper. One can, however, as we have already indicated, find some incidental values coming from an attack such as this. First of all, unusual precautions are generally recognized as necessary in this branch of research. This is due partly to the incredibility and revolutionary character of the results and partly to their exceptional importance. The higher the value, the heavier the guard! With no formal schooling for psi research as such and with little organization in the field as yet, there is naturally a problem of maintaining the highest standards of precaution reached in some of the experiments. There may, of course, be better ways of alerting research workers to the continuing need for the exceptional research standards which our investigations require (that is, better ways than inviting the efforts of such critics as Hansel). But at least something can be said for the "bogeyman value" of his type of activity in parapsychology.

A less obvious service, however, may be credited to Hansel's critique—one that may have an important bearing on parapsychology at this stage. We may well surmise that there are many others than Hansel who feel suspicious of the psi investigations—even though perhaps more judiciously so than he—who suspect that a combination of loose test conditions and moral weaknesses on the part of subjects and even of experimenters could probably account for these rather undigestible results. We submit that it may be a good thing for such people to have an occasional spokesman. Some years ago Dr. George Price (after acknowledging that only this alternative remained) boldly expressed his grave doubts about the honesty of parapsychologists in general.[1] By the time the discussion had died down, it was considered by many parapsychologists that a large amount of much-needed education had been accomplished by the exchanges. Giving the silent skeptics a voice may serve for many of them to bring the issues out of the clouds of vague uncertainty and down to a more solid level of appraisal and judgment that they can appreciate.

[1] Price, G.R. Science and the supernatural. *Science*, Aug. 26, 1955.

But a more subtle value can be claimed for Hansel's sort of criticism. It concerns the question of whether or not parapsychology is really yet a science and, if it is, how much of a unit among the sciences it has become. Criteria of judgment on such a large question are extremely vague and ill-defined. Probably as decisive a factor as any affecting the general status a new field eventually reaches is the outcome of the various challenges presented to its methodology. Most critical students suspend judgment to see how well the new claims stand up under critical attack upon the methods that produced them. We in parapsychology can well afford to welcome these attempts to hack down the structure of our evidence, even if the criticisms are not always in keeping with the best scientific standards (and they seldom are). Radically new researches must be ever ready to stand trial with the expectation that the court is adversely biased and that the quality and quantity of the evidence must therefore be exceptionally strong. But our point here is that contests such as the one Hansel's critique has initiated must be recognized as part of the clumsy and wasteful way by which the test of survival is administered to an emerging radical discovery.

It may be of some advantage to see such a paper as Hansel's against the perspective of history. Some of us in the field today can recall the time when psychical research was a shadowy area in which the serious investigator felt the need of a good detective and when some workers thought that they must be trained in the arts of conjuring. Psychical research was at that stage something like the process of winnowing a few grains of probable fact from great masses of the chaff of human trickery and gullibility. That was a measure of its remoteness from an adequate scientific methodology. Over the years, however, the detective and the magician have left the scene completely, and no one (unless Hansel) would any longer miss them. The methods themselves, after the manner of science, have taken over the problems the detectives were to have solved. It was even in one of the *earliest* volumes of the *Journal of Parapsychology* (Vol. 2, p. 151) that the statement was made that the good faith and morality of the subject in an experiment were no longer a proper concern of the research worker; that if the methods were not adequate to deal with the subject who might be *assumed* to be dishonest, they were not adequate for the standards of a proper

scientific field. Today it is important to hold to the high level of standards that have been achieved and to continue to do so until the understanding of the phenomena brings them more readily under predictable control, until the development of test apparatus takes the burden of safeguarding off the experimenter completely, and progress in the findings reduces the incredibility of psi phenomena to the level of science as a whole. Only at such a time, however distant it may be in the future, can the standards of parapsychology be lowered to those of the other psychological sciences (in which the question of the *honesty* of those involved is not raised).

The possibility of fraud is, of course, by no means confined to parapsychology, and it would even be difficult to say that it is greater or more likely to occur in this branch than in other branches. The challenge of psi and the importance of finding any possible alternative explanation have so exaggerated this hypothesis that it is difficult now to see it with adequate detachment.

It so happens, however, that the topic has been treated recently in a much more general context. Discussing the subject of morality in science in an address given at the 1960 A.A.A.S. annual meeting, Sir Charles P. Snow said, "We have all heard of perhaps half a dozen open and notorious ones [cases of fraud] which are on the record for anyone to read—ranging from the 'discovery' of the L radiation to the singular episode of the Piltdown man." Later he adds, "But the total number of all these men is vanishingly small by the side of the total number of scientists. . . . Science is a self-correcting system. That is, no fraud (or honest mistake) is going to stay undetected for long. There is no need for an extrinsic scientific criticism, because criticism is inherent in the process itself. So that all a fraud can do is waste the time of the scientist who has to clear it up."

Here Sir Charles is speaking of the value to the scientific process itself of "extrinsic criticism." With that we can heartily agree. Indeed, so far as parapsychology is concerned—and it has probably been concerned with a great deal more criticism proportionately than has any other branch of inquiry—it owes nothing that we can discover of its scientific advance to the extrinsic critic. It owes a great deal to the kind of critic who proceeds to do (or to help to do) a better experiment than the one in which he observed a flaw.

But in the present commentary on critics such as Hansel, we have in mind still other values beyond those of the improvement of research methods and the advance of discovery itself. Science-in-the-large is more than its research procedures alone. However adequate its methods may be, the progress of a branch of science may be brought to a veritable standstill if a sufficiently adverse climate of opinion prevails. If parapsychology is to continue to advance, it will have to deal competently not only with its problems of research methods but with its difficulties arising out of the currently prevailing idolatry of mechanism as well—difficulties of support, personnel, acceptance, and the interpretation of its larger meaning.

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A CRITICAL ANALYSIS OF THE PRATT-WOODRUFF EXPERIMENT

By C. E. M. HANSEL

INTRODUCTION

If the design of an experiment is such that the result could have arisen by normal means, the experiment, by itself, cannot provide conclusive evidence for ESP. The result of the Pratt-Woodruff experiment[1] could quite easily have been brought about if the two experimenters had indulged in a trick. Thus, in itself, the experiment cannot prove the existence of ESP. The same can be said of any single experiment which purports to prove the existence of any new process. If the findings of this experiment had been confirmed by all other pairs of experimenters who cared to repeat it, the hypothesis of trickery on the part of the experimenters would itself have become unlikely, and at the same time any skeptic could have repeated the experiment and satisfied himself of the result so that eventually criticism would have tended to disappear. In the absence of confirmation of results, as is normally required where findings clash with existing knowledge, it becomes difficult to design an experiment which can conclusively demonstrate the existence of ESP.

It is to some extent the duty of the critic to repeat an experiment to which he raises these objections, if it has not been repeated by anyone else. But before going to the trouble of doing so he may wish to satisfy himself that he is not wasting his time. If he finds, on examining the experiment, evidence to support an alternative hypothesis to ESP, he would not regard the experiment as worth repeating. Thus we may postulate that an experimental result might have arisen in a particular manner which does not involve ESP, but as a second step we may investigate the experiment to see whether there is evidence that the results were, in fact, brought about in the manner postulated. If we find clear evidence that a particular form of error—or trickery—was responsible for the result there is little point in repeating the experiment.

[1] Pratt, J. G. and Woodruff, J. L. Size of stimulus symbols in extrasensory perception. *J. Parapsychol.*, 1939, **3**, 121-59.

In the case of the Pratt-Woodruff experiment, I would not consider the fact that the result could have been brought about by means of a trick involving both experimenters as sufficient in itself for rejecting it. If, on the other hand, trickery on the part of only one of the experimenters was sufficient, this constitutes a more serious criticism, as the experiment was designed to exclude such a possibility.

I have, in fact, found trickery on the part of one person to be sufficient to account for the result and the experiment could be discarded for that reason alone; but the possibility of such a trick having been used is further supported by characteristics found to be present in the score sheets which are difficult to explain in terms of any other hypothesis including that of ESP.

The Procedure Used in the Experiment

The essential features of the experiment which are pertinent to the present discussion are as follows: The subject and experimenter sat at opposite ends of the table. A screen was placed between them. It was 18″ in height and 24″ in breadth. There was a gap 20″ in breadth and 5″ in height at the bottom of this screen. Five cards (called "key" cards) bearing different symbols were placed in a row on pegs above the gap so that they were visible to the subject but not visible to the experimenter. Five blank cards were placed on the table below the gap so that they were visible to both the subject and experimenter. Each blank card was beneath one of the key cards. A second low screen on the experimenter's side of the gap was positioned so as to enable him to see the five blank cards, but to preclude the subject seeing the experimenter or the cards he was handling. The pack of ESP cards was held face downwards by the experimenter, and during each run he distributed them into five piles opposite the five blank cards in accordance with the subject's guesses. The subject denoted his guess by touching with a pencil the blank card which was situated beneath the relevant key card.

The testing procedure is described as follows (p. 127):

> One experimenter, Woodruff, and the subject sat facing each other across a table . . . The second experimenter, Pratt, sat about six feet from and almost directly behind the subject. The screen was placed in position. While Woodruff shuffled and cut the pack of cards to be used,

Pratt took the key cards from the pegs and handed them to the subject who changed their order and replaced them without giving Woodruff any indication of the new arrangement. In the last sub-series, Pratt rearranged the key cards and put them on the pegs himself; during that period the experimenters were careful that the shuffling and cutting by Woodruff were not completed until Pratt had returned to his usual position, so that there could be no possibility of his seeing any of the cards held by Woodruff after they were shuffled. Woodruff then gave the signal to start and the subject proceeded to indicate his "guesses" by pointing to the blank cards in the opening under the screen. Woodruff distributed the cards following the subject's pointer, but he was in complete ignorance throughout the run of the symbol designation intended by the subject.

At the end of the run, the screen was left in position on the table while Woodruff recorded the actual distribution of the 25 cards on the appropriate record sheets and while Pratt recorded the order of the key cards on his record sheet bearing the same number. [Record sheets had been registered and serially numbered before the experiment.] The order of the key cards was recorded by Pratt in reverse order so as to make them correspond with Woodruff's record when the two sheets were juxtaposed later for checking. Pratt in addition recorded the name of the subject, the type of test, the date, and the initials of the experimenters. This recording was done without any communication between the experimenters or from the subject.

When Pratt finished his record, he carried it to the experimental table. Woodruff had usually finished his recording by this time. In case he had not, Pratt was careful to keep his record out of Woodruff's visual field until the other record was completed. Woodruff then clipped together the two independent records with the common serial number and deposited them without further marks or observation of the sheets themselves in a special locked box provided by the secretary for the purpose.

The screen with the key cards still on the pegs was then laid on its side, either by the subject or by Pratt, so that both the key cards and the 25 cards as distributed were visible to all three persons. Pratt then proceeded to sort out the hits from each pile, laying them nearer the key cards and counting aloud the number of hits for the run. This process was observed by Woodruff and the subject. The hits as segregated were then re-examined and re-counted. The score for the run as thus determined at the time from the cards themselves was recorded immediately by each experimenter in his personal record book.

To continue the test, the screen was again raised to its vertical position, the key cards were rearranged upon the pegs, Pratt returned to his seat behind the desk, and Woodruff, having shuffled and cut the pack of cards, gave the subject the signal to begin.

Other precautions were taken during the experiments which are not discussed here as they are irrelevant to the present argument. These may be seen listed in the original report.

Results

Although the total score for the group of 32 subjects was significantly above chance, only five of the individual subjects obtained scores which were themselves significant at the 0.05 level. One subject, however, obtained a result which yielded a probability of chance occurrence of 4.6×10^{-8}. The mean score for the group was 5.21 hits per 25 trials as compared with the chance expectation of 5.0. The probability of this result arising by chance (allowing for optional stopping) is given as 5×10^{-6}.

Analysis of the Experimental Conditions

When examining an experiment of this nature in order to see whether it is foolproof, we first assume that ESP is impossible and then seek some other cause of the high scores. Wherever a result has a very low probability of chance occurrence we may be reasonably certain that something has caused it to arise. The most important results to consider in this experiment are: (1) the over-all score obtained in the experiment ($P = 5 \times 10^{-6}$); (2) the score achieved by one subject, P.M., ($P = 4.6 \times 10^{-8}$).

If any form of trick was used we should expect its effect to be most easily detected in the case of the high-scoring subject, but the fact that four other subjects obtained scores significantly above the chance level ($P < 0.05$) makes it likely that a trick, if used by the subject, was used by more than one subject. An investigation of the experimental conditions, however, makes it difficult to see how a trick could have been used by any subject provided the experimenters were carrying out their duties effectively. In addition, the high scores of the one subject at some sittings could only have arisen through a trick which enabled two or more extra cards to be known in each run. Thus, if, for example, the cut before each run tended to bring certain symbols to the top of the pack and other symbols to the bottom, such an effect, combined with knowledge of it consciously utilized by the subject or combined with calling tend-

encies on the part of the subject, could not have produced sufficient hits to account for the result.

If the experimenter who handled the pack of target cards carried out his duties efficiently, it is difficult to see how high scores could have arisen through deliberate cheating by any other person involved in the experiments. If on the other hand this experimenter had any knowledge of the positions—or likely positions—of the key cards, he was clearly in a position to affect the result, since he was screened from sight of other persons and had ample opportunity, either while distributing the cards or while recording them, of changing the distributions of symbols in the five piles so as to bring about high scores.

An examination of the orders of the key cards on successive runs showed that they were by no means randomized. Thus when the position occupied by each key symbol in a run is tabulated against its position in the previous run, the contingency table shown in Table 1 is obtained for subject P.M. Thus the experimenter would see, for example, which symbol occupied position 5 at the end of a run and might know that it would most likely be in position 1 during the next run. Such a procedure is, however, difficult to put into operation. Moreover it would be difficult to account for scores

Table 1

CONTINGENCY TABLE SHOWING POSITION OF EACH KEY CARD AGAINST ITS POSITION IN THE PREVIOUS RUN FOR SUBJECT P.M.

Position in Previous Run	Position in Run 1	2	3	4	5	Total
1	27	42	37	33	16	155
2	24	33	19	43	36	155
3	33	29	32	27	34	155
4	21	26	37	25	46	155
5	50	25	30	27	23	155
Total	155	155	155	155	155	775

Taking the expected frequencies in each cell as 31
$\chi^2 = 54.9$; $n = 16$; $P = < 0.00001$

of the size achieved by P.M. at some sittings. Also, as each subject replaced the cards himself on the pegs, the form of the contingency table would probably change markedly with different subjects.

When testing out this possibility, I found it far simpler to obtain high scores when acting as experimenter by noting the symbol occupying one of the end positions and detecting the position at which it was replaced on the pegs. I instructed an assistant to take the key cards from the pegs in order from left to right and then to replace them in different positions on the pegs. It was then quite easy to note the position at which the cards were replaced. This was done by listening to the sound of the cards being replaced and by observing the shadow of his arm on the table under the slit at the bottom of the screen. The positions of the first and last cards replaced were easiest to identify in this way.

If the experimenter had wished to influence the results in this manner he would have adopted the following procedure:

1. During the scoring at the end of a run he would have noted the symbol occupying position 1 (or position 5) in the row of key cards.

2. When the key cards were removed from the pegs by Dr. Pratt he would have noted whether they were removed in order from left to right or right to left.

3. When the subject "changed their order and replaced them" he would assume that the last card replaced would correspond with the card which occupied positions 1 or 5 in the last series of key cards (depending on whether they had been removed from the pegs from left to right or from right to left. This would arise if the subject took the key cards from Dr. Pratt and replaced them in different positions on the pegs without shuffling them.

4. He would have determined the position at which the first or last card was replaced.

5. While recording the cards in the piles at the end of the run, he would have exchanged cards so that extra hits were obtained in the pile for which he knew the identity of the key card.

Any effects of such a procedure should manifest themselves with the best possibility of being detected in the records of the high-

Table 2

POSITIONS WHICH HIGH-SCORING SYMBOLS OCCUPIED IN THE ROW OF KEY CARDS USED FOR THE PREVIOUS RUN IN THE DATA OF SUBJECT P.M. (S = STAR, R = RECTANGLE, P = PLUS, W = WAVES, C = CIRCLE)

Date and Type of Sitting	Run	Score on Run	Highest Scoring Symbol	Hits on Symbol	Position of Symbol in this Run	Position of Symbol in Last Run	Critical Ratio for Sitting
Nov. 21, 1938	3	6	S	2	3	1	
	5	6	P	2	4	1	
	6	6	R	3	4	5	0.0
STM	12	10	W	4	2	5	
	14	8	S	3	3	5	
Nov. 28, 1938	2	7	P	3	1	5	
	3	8	W	3	1	5	
	4	6	C	2	1	5	
	5	6	R	3	1	5	
	6	6	C	3	4	2	
STM	8	9	C	4	3	3	4.85
	9	9	S	4	3	5	
	10	9	W	3	1	5	
	11	7	W	3	4	1	
	12	11	R	5	4	5	
	14	11	P	4	2	1	
	15	6	P	3	1	2	
Dec. 12, 1938	6	10	S	4	3	5	
	7	7	R	3	1	5	
STM	8	11	W	4	4	5	3.83
	10	6	C	3	5	5	
	12	10	C	4	1	5	
	15	8	R	4	1	5	
Jan. 9, 1939	2	6	C	3	3	5	
	3	7	P	3	1	1	
	6	10	C	4	5	1	
	8	7	C	3	4	4	
STM	12	6	S	3	5	1	2.13
	18	6	C	3	4	1	
	19	10	W	4	1	1	
	20	7	C	4	1	5	
	22	9	R	4	2	1	
Jan. 31, 1939	3	10	W	4	1	5	
	4	6	C	2	2	3	
	5	7	S	3	1	5	
STM	7	8	P	3	4	1	0.96
	11	7	S	3	2	1	
	13	6	W	3	4	3	
	15	6	R	2	3	1	
Feb. 3, 1939	2	6	C	3	4	1	
	7	6	P	2	5	2	1.18
STM	10	7	W	4	3	1	
	15	7	R	3	3	1	

Date and Type of Sitting	Run	Score on Run	Highest Scoring Symbol	Hits on Symbol	Position of Symbol in this Run	Position of Symbol in Last Run	Critical Ratio for Sitting
Feb. 10, 1939	2	7	S	3	3	4	
	15	7	P	3	3	2	
	16	9	S	3	1	2	
BSTM	17	7	R	3	2	3	0.57
	22	8	R	4	1	3	
	24	7	S	3	4	5	
	27	10	S	3	2	4	
Feb. 17, 1939	8	8	R	4	3	2	
	9	6	C	2	5	2	
BSTM	11	12	P	4	2	1	1.27
	16	7	C	3	1	5	
	25	7	W	3	3	4	

scoring subject P.M. I have therefore analyzed the record sheets of this subject in the following manner:

1. We take all runs made by this subject in which a score of above 5 was obtained.

2. We note the symbol which secured the maximum number of hits in each such run. Where two or more symbols secured an equally high number of hits we reject that run for the purposes of the present analysis.

3. Having identified the symbol which secured the maximum number of hits in a run, we check its position among the key cards (1-5) used for the previous run.

4. On the null hypothesis we should expect the highest score to arise on a symbol which previously occupied positions 1 or 5 on 40% of occasions, and for it to arise on a symbol which previously occupied positions 2, 3 or 4 on 60% of occasions.

The data from which this analysis is made are tabulated in Table 2. This contains all cases where the subject P.M. obtained above-chance scores and where a particular symbol obtained a maximum of hits.

In Table 3 it will be seen that out of 55 cases considered, there are 39 cases where the high-scoring symbol previously occupied positions 1 or 5 in the key card order and 16 cases where it occupied positions 2, 3 or 4. The following contingency table is obtained:

Table 3

POSITIONS OCCUPIED DURING PREVIOUS RUN, BY SYMBOLS SECURING MAXIMUM NUMBER OF HITS IN PRESENT RUN

Previous Position of Symbol	Cases Observed	Cases Expected
1 or 5	39	22
2, 3, or 4	16	33
Total	55	55

$\chi^2 = 21.89$; $n = 1$; $P < 10^{-4}$.

The three sittings at which the result was significantly above chance at the 0.05 probability level may be compared with the remaining five sittings. The result is shown in Table 4.

Table 4

COMPARISON OF SITTINGS AT WHICH RESULT IS SIGNIFICANTLY ABOVE CHANCE LEVEL ($P = 0.05$) AND REMAINING SITTINGS

Previous Position of Symbol	CR > 2	CR < 2	Totals
1 or 5	23	16	39
2, 3, or 4	4	12	16
Total	27	28	55

$\chi^2 = 3.98$; $n = 1$; $P < 0.05$ (with Yates correction)

Thus while the tendency to obtain high scores on symbols previously occupying positions 1 or 5 is present in both cases, it is present to an increased extent at those sittings in which the higher scores were obtained.

There were, in all, five subjects whose scores were significantly above the chance level ($P < 0.05$). Table 5 shows the number of cases in which the high-scoring symbol occupied each of the five positions in the key card order for each of these subjects when the STM procedure was used.

Taking the combined results for subjects other than P.M, it is found that the effect is mainly confined to position 1. If the data are tabulated so that each sitting for each subject is shown against

Table 5

POSITIONS OCCUPIED DURING PREVIOUS RUN BY HIGH-SCORING SYMBOLS (HIGH-SCORING SUBJECTS, STM PROCEDURE)

Previous Position of Symbol	SUBJECT					Total
	C.C.	D.A.	D.L.	H.G.	P.M.	
1.....	13	2	2	13	16	46
2.....	3	1	4	5	3	16
3.....	6	2	2	5	3	18
4.....	3	0	3	6	1	13
5.....	5	0	5	3	20	33
Total.....	30	5	16	32	43	126

Taking the expected frequencies as 25.2 for each position in the total column, $\chi^2=34.8$; $n=4$; $P<10^{-6}$.

the date on which the sitting took place, the following table is obtained:—

Table 6

POSITIONS IN PREVIOUS TRIAL OCCUPIED BY HIGH-SCORING SYMBOLS (SITTINGS FOR EACH MONTH SHOWN SEPARATELY)

Month and Year	POSITION OCCUPIED BY SYMBOL IN PREVIOUS RUN					Total
	1	2	3	4	5	
Oct., 1938....	7	3	3	2	2	17
Nov., 1938....	6	5	3	1	10	25
Dec., 1938....	1	1	3	0	11	16
Jan., 1939....	26	6	9	10	10	61
Feb., 1939....	6	1	0	0	0	7
Total.....	46	16	18	13	33	126

From this it appears that extra hits were obtained mainly on the symbol which previously occupied position 1, except from Nov., 1938, to the last sitting held in December, when hits were obtained on the symbol which previously occupied position 5.

Results for 1938 and 1939 are compared in Table 7.

THE BSTM SERIES

The BSTM runs were commenced in February after the STM series was completed. At these sittings the system for changing

Table 7

POSITION OCCUPIED BY HIGH-SCORING SYMBOL IN PREVIOUS RUN FOR SITTINGS HELD IN 1938 COMPARED WITH THOSE HELD IN 1939 (HIGH-SCORING SUBJECTS, STM PROCEDURE)

Observed Cases

Position in Previous Trial	1938	1939	Total
1	14	32	46
2	9	7	16
3	9	9	18
4	3	10	13
5	23	10	33
Total	58	68	126

$\chi^2 = 10.7$; $n = 2$; $P < 0.01$ (grouping positions 2, 3, 4)

the positions of the key cards was changed. In the report we read that during the STM procedure "Pratt took the key cards from the pegs and handed them to the subject who changed their order and replaced them." This statement is made when describing how the sitting started. Later when describing the procedure before subsequent runs were commenced we read that "the key cards were rearranged upon the pegs." It is not fully clear whether they were removed from the pegs by Dr. Pratt, as before the first run, or merely changed around on the pegs.

In the case of the BSTM procedure we read, in connection with the first run, that "in the last sub-series, Pratt rearranged the key cards and put them on the pegs himself." We are not told exactly what he did nor whether he adopted the same procedure for all the subsequent runs. If on only a few occasions he took the cards off the pegs from left to right (or right to left) and replaced them in different positions, or if he merely moved the cards around on the pegs, extra hits could have been arranged by the experimenter. Thus, if an attempt is made to obtain two extra hits in every run by exchanging two cards between the piles, and the cards are shuffled before being replaced on the pegs on seven occasions out of the eight, a score similar to that obtained in the experiment may be

brought about.[2] When the cards are shuffled scores will be, on the average, at the chance level, and extra hits will be obtained when the shuffle is not made.

It will be seen in Table 2 that in the case of subject P.M. there is no tendency for the high-scoring symbol to occupy positions 1 or 5 on the previous run, during the BSTM sittings. When the results for all the high-scoring subjects under BSTM conditions are combined the following contingency table is obtained:

Table 8

PREVIOUS POSITION OF HIGH-SCORING SYMBOL FOR BSTM CONDITIONS (14 SUBJECTS)

Position	Observed Number of Cases
1	8
2	14
3	5
4	22
5	21
Total	70

The distribution is by no means random: $\chi^2 = 16.4$; $n = 4$; $P < 0.01$

As the BSTM series commenced after the STM series was completed, there is no reason why a further modification should not have been made so that extra hits were obtained on key cards occupying other positions than 1 and 5 among the key cards.

RANDOMIZATION OF THE KEY CARDS

The possibility of the experimenter obtaining knowledge of the positions of one or more of the key cards obviously depends on whether the key cards were shuffled before being replaced on the pegs. Examination of Table 1 shows that randomization was certainly incomplete in the case of the sittings carried out with subject P.M. The values shown in Table 1 might well arise if the order of the key cards was changed by the subject when he replaced the cards. It may be argued that such values might arise through shuffling habits; but further data make it appear very unlikely that the

[2] Chance expectation of hits where n cards are displaced in a run of 25 trials $= 5.0 \times 0.85n$.

Table 9

ARRANGEMENTS OF KEY CARDS IN SUCCESSIVE RUNS WITH SUBJECT D.A.

Jan. 11, 1939						Feb. 1, 1939						Feb. 22, 1939					
Run	Position of Symbol					Run	Position of Symbol					Run	Position of Symbol				
	1	2	3	4	5		1	2	3	4	5		1	2	3	4	5
1	P	S	C	R	W	1	S	P	W	R	C	1	R	C	W	S	P
2	W	P	C	S	R	2	C	R	W	P	S	2	R	P	C	W	S
3	P	S	C	R	W	3	C	R	W	P	S	3	W	S	R	C	P
4	W	C	R	P	S	4	S	W	C	R	P	4	S	P	C	R	W
5	W	C	R	P	S	5	S	C	R	P	W	5	C	W	P	R	S
6	S	P	R	C	W	6	C	R	S	P	W	6	P	R	C	S	W
7	C	R	W	P	S	7	C	R	P	W	S	7	S	W	R	P	C
8	S	P	W	R	C	8	C	R	P	W	S	8	S	C	P	R	W
9	S	P	W	R	C	9	W	P	R	C	S	9	P	S	C	W	R
10	S	P	W	R	C	10	C	R	P	S	W	10	W	P	S	R	C
11	S	P	R	W	C	11	C	R	P	S	W	11	W	P	S	C	R
12	S	P	W	R	C	12	C	R	P	S	W	12	C	R	W	S	P
13	S	P	W	R	C	13	C	R	P	S	W	13	C	W	P	S	R
14	S	P	W	R	C	14	C	R	P	W	S	14	S	R	C	W	P
						15	P	R	S	W	C	15	C	P	S	R	W
						16	S	P	R	C	W	16	C	W	R	P	S
						17	S	P	R	C	W	17	C	W	S	R	P
						18	R	P	S	C	W	18	C	S	P	W	R

cards were shuffled on all occasions. The orders of the key cards used in all the runs made by the second highest scoring subject (D.A). are shown in Table 9.

There are, in all, 12 occasions when the order of the key cards remained unchanged from the previous run in a total of 50 runs made by this subject. The probability of this number of cases arising by chance is obviously minute. In fact, the key cards remained in the same positions for the last three runs of a sitting held on January 11 and then were in the same positions again for the first run of the next sitting held more than a fortnight later. For the following two runs, they were in the same order but reversed from left to right. Thus, at these sittings, it would appear likely that Dr. Pratt removed the cards from the pegs and handed them to the subject who replaced them, on some occasions, straight back in order from right to left (or left to right if they were removed from right to left). Had the cards been shuffled, or even cut, before being handed to the subject, it is difficult to see how the runs of similar orders among the key cards could have arisen.

Dr. Pratt has told me that he shuffled the cards before handing them to the subject. He may have done so on most occasions, but it appears unlikely that he did so during the sittings made with D.A. There is no mention in the report of the cards being shuffled, and failure to do so on even a small proportion of runs would be sufficient to explain the results of the experiment.

Conclusions

1. The design of the Pratt-Woodruff experiment permits the main experimenter to create above-chance scores by obtaining knowledge of the positions of one or more of the key cards and changing the distribution of cards in the five piles.

2. Characteristics are present in the record sheets of the high-scoring subject P.M. which would be expected to arise if the main experimenter was influencing the experiment in this way.

3. Similar characteristics, but to a less marked extent, are present in the record sheets of the other four subjects whose results were significantly above the chance level.

4. Under STM conditions high scores tended to arise on symbols which occupied positions 1 and 5 in the key-card order for the previous run. They arose mainly on symbols which occupied position 5 during sittings made in November and December of 1938 and position 1 in the remaining sittings.

5. Under BSTM conditions high scores tended to arise on symbols which previously occupied positions 4 and 5 in the previous run.

6. In the case of most of the subjects (other than P.M.) their results could have been achieved by using such a trick during a small proportion of the runs made.

7. There is evidence in the records of the subject D.A. that the key card randomization was far from satisfactory, and the characteristics present in these records would arise if the order of the key cards was being changed by placing them back in different positions on the board, or if the key cards were merely moved to different positions on the pegs.

Discussion

It is possible to obtain high scores under the conditions of the Pratt-Woodruff experiment by means of a trick. It is clear that

such a trick, if used with care, could be extremely difficult to detect. Thus, if extra hits were arranged on symbols occupying the five positions in the key card series approximately equal numbers of times, its detection using the analysis described above would be impossible. In that case, failing detection by a more elaborate analysis, it could only be said that, as the experimental conditions were not fool-proof, the experiment could not provide evidence for ESP.

The analyses made in this paper are by no means exhaustive, nor as complete as is desirable. They were limited by the time available with the records.

Taking the results obtained with the high-scoring subject P.M. it is difficult to see how an alternative explanation to that of trickery on the part of the main experimenter can be provided. It is possible, however, that there is an alternative explanation which has eluded the writer.

I would like to thank Dr. J. B. Rhine for arranging my visit to the Parapsychology Laboratory and Dr. J. G. Pratt who let me see the record sheets and who has given me every assistance in carrying out the investigation.

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REFUTATION OF HANSEL'S ALLEGATION CONCERNING THE PRATT-WOODRUFF SERIES

By J. G. Pratt and J. L. Woodruff

Introduction

The preceding paper by Mr. Hansel may be unique in scientific literature. It claims to offer evidence on a statistical basis that an investigator cheated in his own experiment. However, his "case" is characterized by logical weakness and by inconsistencies. We list below the main points that we shall develop more fully later on.

I. Hansel quotes our report out of context to create the impression of a weakness in the experimental procedure which did not exist.

II. He makes an issue of the lack of randomness of the key cards in spite of the fact that complete randomness of these permutations was neither claimed by us nor required for the adequacy of our experimental procedure and even though he admits that his experimenter-fraud hypothesis could not be based upon this factor.

III. Hansel's method of analysis is fallacious. It fails to take account of different factors which may explain the statistical significance in his findings, and therefore his interpretation of experimenter fraud is without support.

IV. Hansel claims that his "effect" exists generally in the statistically significant sections of our series, and he argues that Woodruff, as a constant factor in these runs involving a number of subjects, was fraudulently responsible for the extra-chance scores. We shall describe a more appropriate method of analysis for finding out whether the rate of scoring among the five symbols is related to the preceding positions of the key cards. Our analysis shows that a run-to-run effect is *not* found in all of the significant sections of the experiment, but is present only in the work of one high-scoring subject. Thus Hansel's attempt to use the data of the other high-scoring subjects from our series to "confirm" his hypothesis of a trick by Woodruff is a failure.

V. We shall point out a number of respects in which our critic shifts his argument or selects his facts, apparently seeking to give his "evidence" an illusory appearance of strength or plausibility. Some of these unwarranted departures from objectivity should have been apparent to Hansel from a reading of his own paper as it stands; others could hardly escape the notice of anyone with access to the data of our experiment, as Hansel himself had.

One would suppose that any fellow scientist invited to the Parapsychology Laboratory for the purpose of studying the evidence for ESP would have attempted preliminary discussion in private of any questions raised in his mind about our data before submitting a paper for publication. Hansel, however, has chosen to present his case without such discussion. Moreover, he has, without waiting for publication, meanwhile been giving his manuscript wide private circulation. His actions appear to represent a deliberate attempt to discredit parapsychology by any means. Under the circumstances we have no choice but to point out his mistakes in print.

I. Our Procedure for Re-arranging the Key Cards

Near the beginning of his paper, Hansel quotes several sentences from our report. He presents these as if they were complete, original paragraphs cited without omissions. These sentences deal mainly with the method followed in changing the key cards between runs.

Actually, the beginning of the quotation given by Hansel does not form the opening of a new paragraph in our report. He has incorrectly given the impression that he has quoted the full paragraph, whereas he has in fact omitted the first, topic sentence. This sentence in our report reads: "The actual testing procedure for each run may be described as follows." To take the place of our topic sentence, Hansel has substituted his own which is similar to ours, except for the fact that the three key words "for each run" are left out.

Within the quotation given by Hansel, the significant sentence about the method of changing the key cards is: "While Woodruff shuffled and cut the pack of cards to be used, Pratt took the key cards from the pegs and handed them to the subject who

changed their order and replaced them without giving Woodruff any indication of the new arrangement." Hansel recognized that it would not have been possible for Woodruff to use his hypothetical trick when this procedure was followed. To get around this difficulty in the way of his experimenter-fraud hypothesis, he conveniently but erroneously has implied that the description given in our report applied only to the *first run of the session.*

Our report clearly shows that the general description of the procedure applied to each run of the series, and not merely to the first run of each session. Hansel could scarcely have overlooked this fact, since it was the essential part of the topic sentence of the first paragraph he quoted from our report. The omission of that sentence enabled him to misrepresent the conditions so as to lend a superficial plausibility to his hypothesis that Woodruff resorted to a trick. The omission and the misrepresentation could hardly be a mere coincidence.

The extent to which his criticism depends upon this misinterpretation of the procedure is amply apparent from the following statements in his paper:

> ... In the [Pratt-Woodruff] report we read that during the STM procedure "Pratt took the key cards from the pegs and handed them to the subject who changed their order and replaced them." This statement is made when describing how the sitting started. Later when describing the procedure before subsequent runs were commenced we read that "the key cards were rearranged upon the pegs." It is not fully clear whether they were removed from the pegs by Dr. Pratt as before the first run or merely changed around on the pegs.
>
> In the case of the BSTM procedure we read in connection with the first run that "in the last sub-series Pratt rearranged the key cards and put them on the pegs himself." We are not told exactly what he did nor whether he adopted the same procedure for all the subsequent runs.

There was no change in the procedure after the first run of the session. Hansel misread our twenty-year-old report on a simple point of procedure—a point that has never been questioned before.

II. Was the Re-arrangement of the Key Cards Adequate?

Nowhere in our report did we speak of the re-arrangement of the key cards as providing a random sequence of permutations. We deliberately spoke not of *randomness* but of *re-arrangement* (e.g.,

the subject "changed their order and replaced them without giving Woodruff any indication.... In the last sub-series, Pratt re-arranged the key cards and put them on the pegs himself...."). The purpose of having the key cards on the pegs outside of Woodruff's sight was to keep him from knowing their positions during the run. This is all that was claimed. To achieve this purpose it was not necessary that the relationship between the order of key cards for one run and that for the next should be statistically random.

Having raised the randomness question, Hansel concedes that it is not relevant to his fraud hypothesis. Nevertheless, he continues to confuse the issue with it. For example, Table 1 in his paper is an analysis of the run-to-run sequence of key cards in the data of the highest-scoring subject, P.M. In discussing this table, Hansel admits that the results of the experiment could not be accounted for on the assumptions that Woodruff had learned the subject's habits of replacing the key cards and had fraudulently misplaced cards to increase the *probability* of "hits." Having rejected this particular hypothesis, Hansel introduces another type of analysis which does not depend upon the nonrandomness of the key cards. However, he leaves the reader with the false impression that the randomness question is pertinent to his basic thesis.

In fact, a later section of his paper is devoted entirely to this irrelevant topic. There he interprets observed nonrandomness in the key cards for subject D.A. as evidence that they were not "shuffled" before being replaced on the pegs. There is another quite obvious interpretation—one that is entirely consistent with the procedure described in the report. The instruction given the subjects was that they should change the order of the key cards and then place them on the pegs in a different arrangement from the one used in the preceding run. The subjects in the STM series, *after having changed the order of the cards in their hands,* placed them on the pegs *while looking at the symbols on the faces of the cards.* The effort to achieve an order unlike that of the preceding run or an order that Woodruff did not know is quite sufficient to account for a nonrandom relationship in the order from one run to the next.

In the case of subject D.A., there occurred, as shown in Hansel's Table 9, an unusually high number of instances in which the order

of the key cards was repeated from one run to the next. Hansel interprets this as evidence that the key cards were taken from the pegs without shuffling and were handed to the subject, who placed them straight back on without changing the order. This seems an untenable hypothesis. Subject D.A. was (and is) a member of the faculty of the Duke University Psychology Department. The most likely explanation is that he decided to find out for himself the effect of putting the key cards in a certain order, and thus the same order appeared from run to run. If this is the case, he said nothing about it to the experimenters. The experimental method involved locking up the written record of the cards as soon as it was made before the score was checked. This procedure made it unlikely that the investigators would notice that the subject was "trying his own experiment" by repeating certain permutations. But regardless of whether our explanation is the correct one, the fact of the matter is that we did not notice the repetition at that time—and this fact we now find neither surprising nor disturbing.

In Hansel's paper, however, the matter is presented as something sinister. The implication is that, when the key cards were not changed, Woodruff might have been aware of their positions and thus he could have placed cards erroneously to increase the number of hits. Indeed, if Woodruff had been trying to make hits in this manner, he had his best opportunity to do so when D.A. put the cards back in the same order (if he had noticed it was being done).

The repeated key cards in the D.A. data thus offer a method of testing the Hansel hypothesis of experimenter trickery. This test is to compare the rates of scoring in those runs (a) when the order was changed and (b) when it was not changed. This is an obvious thing to do. Why didn't Hansel do it? If he had, he would have found that (a) the 12 runs with unchanged key cards scored low, with only 7 more hits than mean chance expectation; (b) the 21 runs with the changed key cards gave a positive deviation of 21 hits, a higher average. Thus he would have known that the repeated orders were not related to the scores.

Hansel even selects facts out of context to favor his interpretation. He shows that the key cards were in the same position for

the last runs of D.A. on January 11 and again for the first run of February 1. He then offers this fact as evidence that the key cards remained unused on the screen throughout this period. Yet the records show that other sessions comprising a total of 540 runs intervened between these dates and the key cards were re-arranged this number of times. Thus the use of a preferred order by D.A. in his first run of February 1 could not possibly mean that the cards remained unchanged since his last session. Therefore, instead of indicating a flaw in the procedure, this observation suggests that our own interpretation is probably the correct one: D.A. preferred certain arrangements of the key cards; and without telling the experimenters, he sometimes placed the cards back in a chosen order after mixing them in his hands. This did not conflict with the experimental conditions described in our report.

III. The Fallacy of Hansel's Method of Analysis

The question which Hansel has raised may be restated as follows: Is the distribution of the score among the five key cards in a given run dependent upon the position of these symbols in the preceding run? In attempting to answer this question, he relies upon a simple count of the hits in each of the five positions in those runs which gave a total score of more than five.

Hansel's method has flaws which render the results ambiguous and therefore uninterpretable. (1) Counting the *number* of hits in each position fails to take account of any tendency on the part of the subject to have more cards placed in one or more of the piles than in the others. (2) His method overlooks the habits of the subjects in replacing the key cards. (3) His method fails to recognize that subjects might have a tendency to score at a higher rate *by ESP* in one or more of the five positions than in the others, or on certain symbols more than on others. (4) These factors might work in combination to produce an "effect" by his method.

Usually, subjects show a tendency to respond more often to the symbols occupying the three inner key-card positions. If there exists at the same time a tendency for subjects to place the key cards that were in the end positions back on the three inner pegs, these two concomitant habits would account for finding *more hits*

on the symbols that had been on the ends. Thus this hypothesis might explain Hansel's results without the necessity of bringing in either his trickery interpretation or any variation of the ESP hypothesis.

The point we are making here is that one simply cannot tell what his results mean. A further study of the data is needed before one can say whether his figures mean anything at all in regard to a run-to-run effect on the scoring. Only after such an effect has been found to exist on the basis of an adequate method of analysis does one properly consider its interpretation. Hansel appears to have been partially aware of the weakness of his case when he wrote: "The analyses made in this paper are by no means exhaustive, nor as complete as is desirable." Whether or not he knew how truthfully he spoke in those lines, they form an astonishing confession with which to end a critique containing so serious a charge.

What the matter boils down to is this: The statistical significance of the results of our paper is not at issue. Hansel's argument against our findings as evidence of ESP is based upon a misinterpretation of our conditions. In the effort to support the trickery hypothesis which he chooses to offer, he has carried out analyses of the data which yield uninterpretable results. If his criticism has *any* valid bearing on our experiment, we have been unable to find what it is.

We made analyses of the data of the four highest-scoring subjects to find out to what extent the main complicating factors mentioned above actually exist in the records. Table 1 shows the frequencies of *responses* over the five positions and the chi-square evaluation of the distribution for each subject. Only one subject, D.A., shows an approximately even distribution of trials. The other three all show a tendency to point more often to the second, third, and fourth positions and to neglect the two end ones. This tendency only approaches a level of statistical significance in P.M., but it is highly significant in H.G. and C.C.

A second factor to be considered is the distribution of the success rate (percentage of hits) over the five positions. Table 2 shows the results of the analysis for this factor. It is apparent that there is a considerable range of variation in each of the four subjects.

Table 1

DISTRIBUTION OF RESPONSES OVER THE FIVE POSITIONS FOR THE FOUR HIGHEST-SCORING SUBJECTS

Subject	TRIALS IN EACH POSITION					χ^2_4	P
	1	2	3	4	5		
P.M.	756	843	862	803	786	9.05	.06
D.A.	251	249	251	238	261	1.07	.9
H.G.	842	1006	1044	924	859	33.66	$<.000005$
C.C.	672	1074	1148	1128	828	182.22	$<.0000001$

Note: The subscript numerals given with χ^2 indicate degrees of freedom. Thus each χ^2 value in this table has 4 d.f.

While the differences in scoring rate are not statistically significant, they do exist; and they would therefore have to be taken into account in any method for studying a run-to-run relation of key card positions and scoring.

We have not analyzed the two subjects, H.G. and C.C., for tendencies shown in the replacement of the key cards. As we have said, we recognized that the procedure might not generate a random order of permutations, and the evidence presented by Hansel in support of this anticipation from the data of P.M. and D.A. is sufficient to show that this is a real factor that any method would have to take into account.

The amount of effort that Hansel devotes to applying his method of analysis to the other high-scoring subjects after his discovery of the "effect" in P.M. indicates that he felt (though he did not explicitly state) the need for confirmation of the findings. We agree that this need for confirmation is one that must be met before any

Table 2

PERCENTAGE OF HITS TO TRIALS IN EACH POSITION (Chance Expectation: 20%)

Subject	POSITION				
	1	2	3	4	5
P.M.	26.46	22.42	23.55	23.04	21.50
D.A.	25.10	25.70	22.31	18.91	24.90
H.G.	20.78	21.87	20.59	21.43	23.63
C.C.	20.98	23.56	20.03	21.81	21.14

kind of conclusion can properly be reached regarding an *a posteriori* result such as the one he found in P.M.'s data. But his claims that the data of the other high-scoring STM subjects (his Table 5) and of the BSTM series (his Table 8) confirm the "effect" are without any foundation. In the first place, he has not treated the additional data separately in these analyses, but has always loaded the results in favor of his prediction by re-using the P.M. data. In the second place, the two subjects, H.G. and C.C., who superficially gave a suggestion of a run-to-run effect (his Table 5), showed, as we have indicated, a strong favoring of the three inner positions, and therefore they would be expected to get a larger number of hits on them. If Hansel's Table 5 shows anything at all for these two subjects, it may only be that they tended to place the key card that had been in the first position back on one of these three inner pegs. (But we shall show in the next section that these data, when properly analyzed, show no Hansel effect of any statistical significance.)

IV. A More Appropriate Statistical Analysis

An analysis that more adequately meets the requirements for a test of a dependence of scoring upon the key card positions of the preceding run is the following: First, tabulate the hits and misses in five groups defined by the position of the key card in the preceding run. Then test by chi-square the 2×5 matrix of hits and misses to see whether there is any significant variation in scoring rate. We have done this kind of analysis for different groupings of the data with which Hansel was concerned, and the results are as follows:

(1) *P.M. Data for the STM Series, Runs with Scores of Six and Above.* With this subject, there is an effect of such a high significance level ($\chi_4^2 = 31.67$, $P = .000005$) that one is justified in forming hypotheses about it, but not in attempting to reach a conclusion without confirmation or without considering alternative possibilities of interpretation. The contributions to chi-square of the "hit" cells for those two groups having the key cards that were formerly in positions one and five are based on positive deviations, and the other three hit cells all have negative deviations. This analysis, based on the data shown in our Table 3, shows that the run-

Table 3

P. M.'s Results in STM Runs With Scores of Six or More in Relation to Previous Key Card Positions

Key Card Position in Previous Run	Hits	Misses	Total
1	108	194	302
2	66	199	265
3	61	207	268
4	77	205	282
5	125	183	308
Total	437	988	1425

$\chi^2 = 31.67$ (4 d.f.)
$P = .000005$

to-run key card effect in subject P.M. is indeed a striking one that deserves further attention. We shall return to it later.

(2) *The High-scoring STM Runs for C.C. and H.G.* These two subjects, according to Hansel, came closest to providing a "confirmation" of his effect in the P.M. data. By our analysis neither subject alone showed any tendency for the scoring rate to depend upon the previous key card positions: For H. G., $\chi_4^2 = 2.81$ with $P = .6$; for C.C., $\chi_4^2 = 4.30$ with $P = .3$. Combining the analysis

Table 4

Results of H. G. and C. C. in STM Runs With Scores of Six or More in Relation to Previous Key Card Positions

Key Card Position in Previous Run	Hits	Misses	Total
1	228	482	710
2	190	514	704
3	192	486	678
4	198	478	676
5	199	508	707
Total	1007	2468	3475

$\chi^2 = 5.15$ (4 d.f.)
$P = .28$

for these two subjects (see our Table 4) yields results that are still below the level of significance ($\chi_4^2 = 5.15$ with $P = .28$).

(3) *The BSTM Data.* Table 8 in Hansel's paper is especially revealing on the question of the unreliability of his method of analysis. He finds there for the BSTM data a value of χ_4^2 with $P < .01$. Our analysis (our Table 5) gives a chi-square that is *not* significant ($\chi_4^2 = 6.04$, $P = .2$). But even if the BSTM data had supported Hansel's "effect," this finding would have presented strong evidence against his "interpretation." The reason is that the method of rearranging the key cards in this sub-series, when Pratt changed the order and replaced the cards face-inward on the pegs, completely eliminates the question of Woodruff's having been able to keep track of one or more symbols. Yet the rate of hitting in these BSTM records was the same as in the series as a whole, and this fact points to something in the subjects' responses as the explanation. The conditions of the experiment left only the ESP interpretation.

Hansel began his investigation with the data before him and with the conviction that there must be some explanation besides ESP for the results. Searching through the work of the highest scoring subject, he came upon something which he could interpret as evidence of fraud by Woodruff. Because of this groping, "after-the-fact" approach, this initial finding could not be conclusive even if the method of analysis had been adequate. Confirmation of the

Table 5

RESULTS OF BSTM RUNS WITH SCORES OF SIX OR MORE IN RELATION TO PREVIOUS KEY CARD POSITIONS

Key Card Position in Previous Run	Hits	Misses	Total
1	216	561	777
2	219	561	780
3	215	556	771
4	253	525	778
5	234	560	794
Total	1137	2763	3900

$\chi^2 = 6.04$ (4 d.f.)
$P = .2$

effect in the data of other high-scoring subjects was therefore of paramount importance. Hansel's efforts to achieve this objective show that he recognized this need. These efforts failed—as we have shown—in spite of his claims to the contrary. This failure was all the more dismal since he was not consistent in applying the test he used on P.M.'s data, but shifted the basis of analysis in the "confirmatory" applications. This fact, coupled with his repeated use of the P.M. data in the later tests of significance, makes much of his paper downright misleading.

Hansel deals in his analysis with only a small portion of data selected from a large body. Out of P.M.'s series of 4,050 trials, he selects a relationship which depends upon only 55 observations. This creates the likelihood that the criteria for data selection were established after the large body of data had been examined and its internal characteristics noted. As can be easily demonstrated, when such procedures of selection are used, even "random" data can be made to yield highly significant results.[1]

V. Some General Observations and Concluding Remarks

In his "Introduction" Hansel seeks to convey the impression that attempts have been made to repeat the Pratt-Woodruff exper-

[1] To illustrate this point, one of us examined the first block of 1,000 numbers in the Kendall and Smith, *Table of Random Sampling Numbers* in search of samples of 55 consecutive items which would show significant departures from chance and would therefore provide "evidence" that someone used a "trick" in making up the table. Here is some of the "evidence" which was easily found in support of this "hypothesis." (1) In columns 2 and 3 and the first four numbers of column 4, there are 41 odd digits and 14 even digits ($\chi_1^2 = 13.25$, $P = .0003$). (2) Again the sample of 55 digits made up of columns 29 and 30 and the bottom five digits of column 31 contain 15 odd numbers and 40 even numbers ($\chi_1^2 = 11.36$, $P = .0008$). (3) The 40 items in the twenty-fifth row plus the first 15 items of the twenty-fourth row contain 27 of the digits 2, 3, and 4 combined, and only 28 of the remaining seven digits ($\chi_1^2 = 9.54$, $P = .002$). (4) The first two blocks of digits spanning columns 21–24 and the first 15 items in block 3 in these same columns contain 14 fives where only 5.5 are expected and 41 of the other nine digits where 49.5 are expected ($\chi_1^2 = 14.60$, $P = .00014$). (5) Extending the above sample through the fifth block of columns 21–24 shows that the excess number of fives increased still further (for this 100-item sample, $\chi_1^2 = 16.00$, $P = .00006$). Findings of this kind can be obtained at will if one takes almost any body of data and makes selections from it upon the basis of certain criteria which one establishes by inspection and then applies rigorously in selecting a smaller sample. Such a procedure proves nothing except that, when selected in this fashion, even "random" data may have extreme idiosyncrasies. It would have been surprising indeed if a person with the degree of ingenuity demonstrated by Hansel had not been able to obtain some "suspicious" results.

iment without success. In actuality, insofar as efforts to replicate our procedure are concerned, the score stands: Attempts = 0, Successes = 0, Failures = 0! Confirmation is, of course, a different matter. The numerous positive results obtained in ESP research over the 21-year span since the publication of our series constitute, in a very precise and real sense, confirmation of our ESP evidence.

Hansel employs certain "verbalisms" as if they were established facts. Thus, in accounting for the fact that the BSTM data do not reflect the same effect as that found in P.M., he states that ". . . there is no reason why a further modification should not have been made . . ." He fails to specify: (a) why a modification should have been made if the earlier and "easiest" form of "trickery" which he has already offered was so effective; (b) what the "modification" is and how it was accomplished; and (c) what the evidence is to support the existence of such a "modification." Are we to infer that mere supposition is adequate evidence for him when the reputation of a fellow-psychologist is at stake?

Now let us deal with the question: How are the P.M. results to be explained? The existence of an effect in her STM data, even when the matter is examined by a proper method, still leaves something of a question for which an answer is in order and which may be considered even though a final answer is not now within reach. Hansel has too eagerly offered only the hypothesis of trickery. Other hypotheses which have been or can be considered are:

(1) Actually, one can offer a consistent and reasonable ESP hypothesis, as follows: For the subject P.M., the run began, in the psychological sense, when she re-arranged and placed the target cards. The ESP task being a difficult one, she dealt with it by a "narrowing of attention" procedure. For her the task became one of attempting to identify only *some* of the cards in the deck: those with the particular symbols which had become salient because of their prominent, end positions in the preceding run.

(2) There may be an alternative ESP interpretation, such as a differential rate of scoring on the five symbols coupled with some habitual tendency in the placement of the symbols on the pegs.

(3) Finally, as stated above, this may be a selected, meaningless, statistical effect, for statistical oddities are a dime a dozen. To take

one seriously it is necessary to confirm it. The data of other subjects in our series fail to support this oddity, whereas they do support the significant scoring level of the experiment. Therefore the Hansel effect is still unconfirmed and unexplained, and it certainly could not explain the Pratt-Woodruff results.

It must, of course, be recognized that conjectures of possible ESP functioning to account for this new effect suffer the same shortcoming as does Hansel's "trickery" hypothesis: they are offered in relation to highly selected data, and the same data which led to the formulation of a hypothesis cannot be used to verify it. The advantage of the ESP hypothesis lies in the fact that it allows for differential results for different subjects while Hansel's, involving alleged trickery by an experimenter who was always present, requires consistency for its verification. Thus, for verification of his hypothesis, as has been pointed out, (a) all high-scoring *subjects* should show the Hansel effect, which they do not; and (b) all high-scoring *series* should show the Hansel effect, which they do not. (At the point where Hansel says, regarding the supposed method of trickery, that "there is no reason why a further modification should not have been made," he was attempting to justify his hypothesis as the explanation for a different kind of effect in the BSTM data. His effort was uncalled for, however, because there was no statistically significant "effect" in the BSTM data to be explained).

We who have worked in parapsychology have learned not only to operate with special safeguards, but to expect a degree of suspicion. However, we are entitled to *responsible* questioning and to *mature* consideration of the evidence. What experience has Hansel had with his professional fraternity in psychology that makes it almost (with him) a foregone conclusion that Woodruff was a conscious cheat? If Hansel is a sincere student of our branch of science, he must know that since our paper was published in 1939 Woodruff has been either author or co-author of eight published psi research reports. Of the eight, only one reported total results which deviated significantly from chance. Three reported no significant results of any kind. Simple failure to publish these three non-supportive (for the ESP hypothesis) papers would have been

a less blatant form of bias than that which Hansel has alleged. Other specific details of these pieces of research and about the published articles could be cited as evidence of objectivity. Our critic may, of course, attempt the task of furnishing a similar dossier of his objectivity.

In the last analysis, it becomes apparent that the existence of psi phenomena does not depend upon what the parapsychologist believes or what the critic believes. It is also apparent that this kind of controversy settles nothing insofar as the basic question is concerned. We find ourselves in substantial agreement with Hansel's contention that it is most difficult to arrange a test of ESP which will rule out the possibility of some form of fraud[2], so charges of fraud have been and will continue to be leveled against investigators who report positive results. Parapsychologists must accept this fact and only those who are temperamentally suited to handle this kind of charge should attempt parapsychological research and publication.

But does the critic not also have an obligation? Criticism comes easily; research is more difficult. Let the critic repeat the research he criticizes. Let him introduce the modifications he feels are needed. The danger for him lies in the possibility that he will find supportive evidence for ESP. Then he too will face a test of his objectivity in whether he publishes or not. If he passes this test, he may expect to be faced with the same allegations of fraud that he has imposed on others.

As a rule, one scientist does not question another's good faith. Research workers in parapsychology have taken more notice of the possibility of fraud in the investigator than is the case in any other branch of science. The reason is not that fraud has been found more often in this branch, but rather that the revolutionary impact of the findings upon current scientific theories has for some diehard skeptics made even this extreme alternative attractive as a possible escape from accepting the conclusions of the parapsychologists. Thus the raising of the question of fraud by the experimenter is only the ultimate stage of criticism: the final, reluctant recognition of the challenge of the field and of the strength of the evidence. Parapsychologists can therefore accept the need to be on guard both

[2] This is not a novel position for us, as we discussed the question in this vein in our original report (p. 140).

against fraud itself and against unwarranted charges of fraud as normal professional hazards on their frontier of science. The final answer to allegations such as the one Hansel has made is in the *number* of independent confirmations. In the two decades since our experiment was reported, the structure of the evidence for ESP and for psi in general has improved in both quality and quantity. And in the large, well-buttressed edifice that the total evidence now forms, we think the section of the foundation identified as the Pratt-Woodruff series is still firm.

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RUN SALIENCE IN THE PRATT-WOODRUFF SERIES

By J. G. Pratt

ABSTRACT: A re-examination of the Pratt-Woodruff Series (1939) has revealed that the four highest-scoring subjects had results that were higher at the beginning and ending of the run than in the middle. Three of the subjects showed a higher rate of scoring both at the beginning and end of the run, while the fourth one reached a high level only at the end. The analysis applied to the results of the four subjects combined indicated that these position effects, which are similar to others found previously in a number of ESP tests, were highly significant statistically.—Ed.

At the time our experiment[1] was carried out and reported, parapsychologists were not nearly so well informed regarding the existence of "secondary effects" in psi test data as they have since become. We made no effort, therefore, to analyze our records thoroughly with the general objective of discovering aspects of the subjects' performance other than those specified in the experimental design.

The current critical concern about our ESP series, as reflected in the critique by Mr. Hansel in this issue of the *Journal,* has had at least one constructive effect. The re-examination of the data has brought to light secondary effects which might otherwise have remained undiscovered in the data for many more years, if not indeed forever. One such effect we pointed out in our reply to the Hansel critique: our procedure apparently engendered in the subject P.M. a psychological set favorable to ESP scoring on one or more of the key cards. Future investigators might well look for similar results in their own subjects. However, it is not possible, as we indicated in our reply to Hansel, to draw any conclusion regarding this run-to-run key card effect on the basis of P.M.'s work alone.

Actually, the current interest in our series was initiated before Hansel's visit. The examination of the data had been started in

[1] Pratt, J. G., and Woodruff, J. L. Size of stimulus symbols in extrasensory perception. *J. Parapsychol.,* 1939, **3**, 121-58.

August, 1960, by Mr. Christopher Scott, who had been invited by the Parapsychology Laboratory for a visit of two weeks. Scott's interest and approach were similar to Hansel's in the sense that he started from a viewpoint of strong skepticism and was primarily interested in attempting to discover some basis other than ESP for explaining our significant results. His interest, however, focused upon the possibility that the subject might somehow have been able to take advantage of nonrandom cutting of the cards by Woodruff behind the screen. I was present at the Laboratory during Scott's as during Hansel's visit, and made the data of the series available for such study and analyses as the vistors wished to make.

Scott's interest was focused primarily upon the question of whether the significant scoring was concentrated in either the *first* or *last* trial of the run, on the assumption that one or the other or both of the two cards involved might have been "hit" by the subject if he knew the point at which the pack was cut. Unfortunately, however, the form in which the records were kept did not make it possible to reconstruct the exact order in which the 25 cards were laid down during the run. The record shows the cards of the ESP pack as they were sorted into five piles corresponding to the key cards. While the cards are most probably in chronological order within each of the five groups, it was not possible to reconstruct the sequence of the subject's responses among the five key card positions.

I pointed out that it *was* possible, however, to analyze the records so as to approximate the subjects' order of response. The card record could be divided into three groups, the first one consisting of the five cards made up of the top card in each of the five positions, the second one consisting of the bottom card recorded in each of the five positions, and the third (middle) group consisting of the remaining 15 cards. On the basis of such an analysis, we could be almost certain that the first group contained the first card which the subject put down, and it would be highly probable that it would contain in most instances the second and third cards as well. It was even likely to contain the fourth and in some instances would contain the fifth response. Similarly, the group made up of the five bottom cards on the record would in all likelihood contain the last card of each run and would contain a large proportion of the

immediately preceding cards in the packs as well.[2] Thus, this type of analysis offered what seemed to be a worthwhile opportunity for examining the data of the high-scoring subjects of our series to see if there was any evidence of position effects within the run, particularly of the beginning and ending salience or U-curve effect which has been so often observed in other ESP records.

The analysis was begun by Scott during his visit, and it has been extended since that time in the Laboratory under my direction to include the work of the four high-scoring subjects. The results indicate that there is indeed strong evidence of run salience in our series. For reasons that will be stated shortly, this effect is not interpretable on any such hypothesis as that which Scott had in mind. I appreciate his interest, which was primarily responsible for bringing this evidence to light, and I am reporting it here as a positive contribution which has come out of the critical attention to our series. In doing so, I do not imply that Scott would concur in my interpretation, and I do not intend in any way to limit his

Table 1

RUN SALIENCE IN THE DATA OF THE FOUR HIGH-SCORING SUBJECTS

	CARDS			
	Top Five	Middle Fifteen	Bottom Five	Total
Hits	705	1877	714	3296
Misses	2260	7018	2251	11529
Total	2965	8895	2965	14825
Percentage (Hits/Total)	23.78	21.10	24.08	

$\chi^2 = 16.57$ (2 d.f.)
$P = .0003$

[2] The statements are true as given here *if* Woodruff turned the whole pile of cards in each position over and then recorded them in order while working down through the face-up group. This would have been the quickest and easiest way to make the record. However, he could have recorded the cards in the reverse order, which would mean that any salience effect found in the record would be reversed, chronologically. We do not think the latter is the case, but in the absence of any specific note in our experimental record covering this point we cannot be positive about it. Uncertainty on this point, however, does not detract from the value of the analysis as an *exploratory* study for run salience in our series.

further studies of the data and his reporting of his own interpretations.

This salience analysis has been carried out for the four high-scoring subjects. The results for the data of all four combined are shown in Table 1, where the hits and misses of the top, middle, and bottom groups from the target record are given in a 2×3 matrix. A chi-square test of these data shows that there is a highly significant departure from the rectangular distribution expected on the "no salience" hypothesis. The percentage figures shown in the bottom row of the table indicate that this significance arises from the fact that there was a higher rate of scoring among the top five and the bottom five cards than among the middle 15. The variation

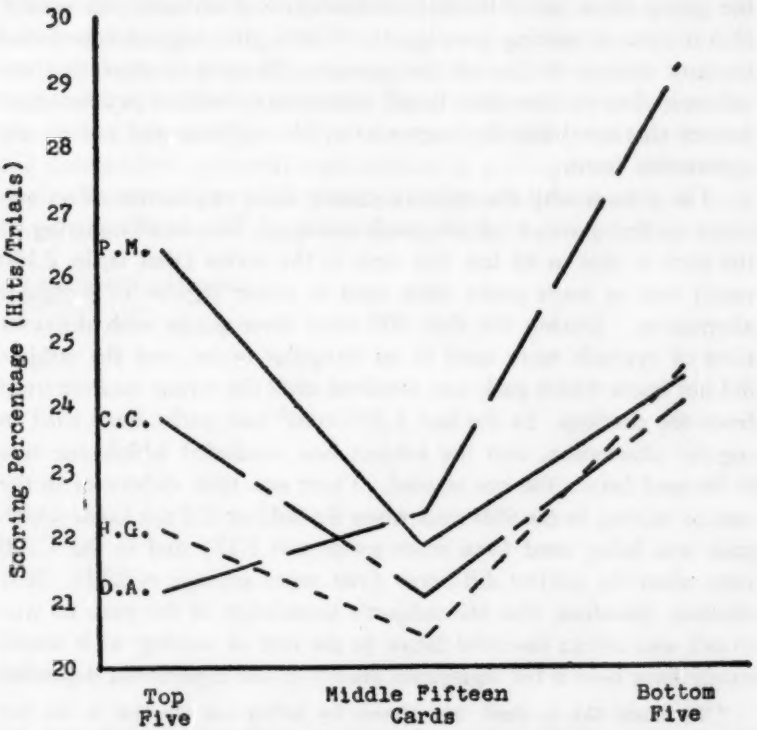

FIG. 1. Individual curves of ESP performance within the run for four high-scoring subjects. The base line is mean chance expectation.

is so large that it would be equalled or exceeded on a chance basis only three times in 100,000 on the average.

It is interesting to observe whether there were striking differences among the four subjects in the run salience effect. Figure 1 presents the distributions for each one separately in terms of the individual percentage rate of hitting in the three groups of trials within the run. Inspection of the curve shows that the results of three of the subjects, P.M., H.G., and C.C., fit fairly closely into the familiar pattern of the U-curve of success. The fourth subject (D.A.), however, does not show any beginning salience in his runs, but very striking ending salience. In fact, the first group of trials are, for this subject, the lowest in rate of scoring within the run, and the middle 15 are only slightly higher. However, in the group made up of the bottom five cards from each run record, D.A.'s rate of scoring goes up to 29.20%, the highest rate found for any subject in any of the groups. Thus it is clear that the salience effect in these data is still related to *individual psychological factors* characterizing the *responses of the subjects* and not to any extraneous factor.

The reason why the salience cannot be a run-to-run effect relevant to the question of the randomness of Woodruff's cutting of the pack is that in all but 300 runs of the series (that is, in 2,100 runs) two or more packs were used in either regular or irregular alternation. During the first 900 runs three packs with different sizes of symbols were used in an irregular order, and the subject did not know which pack was involved until the screen was removed from the checkup. In the last 1,200 runs[3] two packs were used in regular alternation, and the subject was reminded which one was to be used before the run started. There was little difference in the rate of scoring in the 900 runs when the subject did not know which pack was being used (run score average = 5.17) and in the 1,200 runs when the subject did know (run score average = 5.22). It is obvious, therefore, that the subject's knowledge of the pack he was to call was not an essential factor in the rate of scoring, as it would surely have been if the significant results of the experiment depended

[3] We would like to thank Mr. Hansel for calling our attention to the fact that there is a mistake in the opening sentence of the last paragraph on p. 141 of our research report. There the next to the last sub-series is said to have had 1,200 runs, whereas it contained only 800 runs.

on either conscious or unconscious inference based upon observations of nonrandom cutting during preceding runs.

Run salience has been of frequent occurrence in other ESP series and the analysis described above was designed and carried out specifically for the purpose of seeing whether this type of position effect could be confirmed in the data of our series. The evidence presented in Table 1 lends strong support to the run salience effect. This old discovery was not in need of confirmation, however, and I think that the value of the finding lies less in its confirmatory aspect than it does in the suggestions for further research. To make the analysis for run salience in our data, it was necessary to "invent" a method which may prove to be applicable for checking for position effects in the data of matching tests of other experiments as well. If salience proves to be as common in tests of this kind as our results suggest, matching tests might provide a useful tool for the study of this aspect of psi performance. Also, ESP research workers might well consider whether they could not profitably cut down on the more sterile middle trials of the run by making the run shorter and thus achieve a general improvement in scoring level. This suggestion is not new, but it is a tantalizing possibility which is at least raised once more and somewhat strengthened by our findings.

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BOOK REVIEW

THE ENIGMA OF SURVIVAL: The Case For and Against an After Life. By Professor Hornell Hart. Springfield, Illinois: Charles C Thomas; and London: Rider & Co., 1959. 286 pp.

The object of this book is to set before the reader the main reasons which have been adduced by instructed and balanced writers, for and against the view that there are well established paranormal facts which make it highly probable that at any rate some human beings have survived the death and dissolution of their physical bodies. In the above sentence the words "instructed and balanced" must be emphasized. Very few of our contemporaries have devoted to the alleged facts that amount of time and attention which would entitle them to express any opinion whatever about them; and, of those few, a fair proportion have obviously labored under such strong emotionally toned prejudices, in the one direction or the other, that their opinions on the subject are of interest only to students of the psychology of nonrational conviction. There have been and there are, however, enough writers having the necessary qualifications to make Professor Hornell Hart's undertaking very well worth while. He himself is one of them. And one may add (merely by way of illustration) such names as Edmund Gurney, William James, Mrs. Sidgwick, and Mr. Drayton Thomas, in the past, and Professors Gardner Murphy, E. R. Dodds, and H. H. Price, in the present.

Taking the facts as a whole, and allowing for all that can fairly be said on the other side, Professor Hart himself comes to the conclusion that each human individual continues to be conscious and active after the death of his physical body and that this *post mortem* consciousness and activity is sufficiently continuous with the *ante mortem* to make it reasonable to speak of the "survival" of the individual. He does not conceal that opinion, and he stresses the arguments for it, and endeavors to meet the objections to it. But the opposite view, and the arguments in favor of it and against the survivalist-hypothesis, receive very full and fair attention. Anyone who reads the book carefully will be provided with an ample and a

representative selection of data on which to base his own conclusions. That is what the author wants the reader to do.

The contents of the book and the course of the argument may be summarized as follows: Book I is devoted to the evidence for extrasensory perception of one kind or another and to objections which have been raised against that evidence. The first chapter is concerned with typical *experimental* work; the second with evidence (mostly from sporadic cases or from experiments with certain mediums) for noninferential *foreknowledge;* and the third with the evidence which suggests that various kinds of extrasensory perception occur in the course of sittings with *gifted trance-mediums,* such as Mrs. Piper, Mrs. Thompson, Mrs. Leonard, and Mrs. Garrett. There are valuable references to the difficulties in assessing the relevance and the significance of the contents of mediumistic utterances (Mr. Parsons' experiment), and to attempts to devise an objective method of estimating their relevance and significance statistically (Saltmarsh and Soal, Pratt and Birge, and Schmeidler). My own impression may be summarized in the words quoted from an address by Professor H. H. Price in 1958: "The best informed disbelievers in survival believe in the reality of telepathy and clairvoyance. . . . It is no longer possible to reject the reality of *all* supernormal phenomena *en bloc.*"

Book II is concerned with the *prima facie* evidence for survival provided by mediumistic communications. The first chapter in it gives in some detail four fairly impressive cases. These are the ostensible communications from (1) "George Pelham," through Mrs. Piper; (2) Col. Talbot, through Mrs. Leonard; (3) the captain of the crashed air-ship R. 101, through Mrs. Garrett; and (4) young Mr. Aitken, through Mrs. Leonard, in the course of "absent-sitter" séances held by Drayton Thomas at the request of Mr. Aitken senior.

The second chapter is devoted to the communications received by Mr. C. Drayton Thomas in the course of more than 500 sittings which he had under various conditions with Mrs. Leonard between 1917 and 1953. In these the main ostensible communicators were his father and his sister. They may be said to have taken control of the proceedings, often ousting Mrs. Leonard's usual control

"Feda," and ostensibly speaking directly through Mrs. Leonard's mouth. The main types of evidence procured by Drayton Thomas are the following: (1) A detailed analysis of the production and the contents of the *"direct-voice" utterances.* This led him to what may fairly be called a critical but unqualified acceptance of a straightforward survivalist interpretation of the phenomena. (2) An analysis of the many *predictive* messages which he received and found to be fulfilled. (He interpreted them mainly in terms of knowledge by the communicators of intentions already formed by persons still living on earth; of influence by the communicators on such persons, in accordance with their own plans; and so on.) (3) *Proxy sittings,* where he asked as sitter for a person who was often a complete stranger to him, and where he knew practically nothing about the earthly life and circumstances of the ostensible communicator. (4) *Book tests.* It seems to me that Drayton Thomas' work is of very great interest and importance, whether we agree or disagree with his conclusions; and I am glad that Professor Hornell Hart has devoted attention upon it, both in this chapter and elsewhere in his book.

The third chapter is devoted to "cross-correspondences" in the automatic writings, or the oral utterances, or both, of several automatists operating simultaneously without normal intercommunications. This topic is introduced by three simple examples; e.g., (1) "Laurel" and "laurelcrown"; (2) "Death" and "Thanatos"; and (3) the Talbot Forbes Case. These are followed by accounts of two highly complex cases (the second of which is not strictly a cross-correspondence); viz., the Lethe Case and the Ear of Dionysius Case. The author points out that the cross-correspondences have produced very different impressions, as regards their bearing on survival, on equally competent and fair-minded commentators. He quotes Alice Johnson, Oliver Lodge, and Gerald Balfour among others who have been favorably impressed; and Gardner Murphy, Saltmarsh, Dodds, and Hereward Carrington among others who have expressed reasoned doubts as to whether the cross-correspondences can safely be ascribed to the surviving spirits of the eminent deceased scholars who are alleged to have designed and executed them.

Book III is concerned with the arguments of those who, while admitting that the best mediumistic communications contain much that is of paranormal origin doubt or deny that the agency of the surviving spirits of the deceased is involved.

The first chapter is devoted to giving examples of well-established cases of the following kind: communications, purporting to come from the surviving spirit of a certain allegedly deceased person, are given in all good faith by a medium in trance. They contain much relevant detail which the medium could not possibly have known or guessed by normal means; and the voice and mannerisms may be highly characteristic of the person in question, though the medium has never met him or her. Yet it is certain that the alleged communicator either *never existed at all,* or that he was at the time living on earth and going about his ordinary business. Moreover, other ostensible communicators, purporting to be the spirits of certain persons who undoubtedly have lived and died and who have given good *prima facie* evidence of their identity, will often testify to the existence in the spirit world of these purely fictitious or still physically embodied pseudo-communicators.

As examples of purely fictitious pseudo-communicators, Professor Hart gives (1) "Bessie Beals" in Stanley Hall's sittings with Mrs. Piper, and (2) "John Ferguson" in Soal's sittings with Mrs. Cooper. As examples of pseudo-communication purported to come from the surviving spirits of persons who were in fact still living on earth, he gives (1) the Gordon Davis Case (in Soal's sittings with Mrs. Cooper), and (2) the case of Canon Douglas' supposedly dead but actually living French chauffeur, Réallier.

In the second chapter the author summarizes the arguments of the most important writers (in particular E. R. Dodds and Gardner Murphy) who have stressed the difficulties which such cases present to the survivalist hypothesis. They have been inclined to interpret all ostensible communications from the dead through mediums as due to "a comprehensive form of telepathy, clairvoyance, precognition, and retrocognition on the part of the medium, together with unconscious dramatization on her part." They have also emphasized the dependence of the particular form which mediumistic phenomena have taken in the course of history on the relevant be-

liefs and desires prevalent at the time in the medium's social environment.

In Book IV we pass to a very different kind of *prima facie* evidence for survival; viz., the occurrence, on one or more occasions, of *an apparition* which plainly refers to a certain deceased person. This is a topic on which Professor Hart has a special right to speak, since he has devoted many years to studying and reflecting upon the available reports and has embodied his findings in an important paper: "Six Theories About Apparitions," in the S.P.R. *Proceedings* (1956).

In the first chapter the reader is provided with accounts of four striking examples; e.g., the Chaffin Will Case, and the cases of Samuel Bull, Joseph Collyer, and W. E. McBride. (The first and second of these appear to be well attested. I am not greatly impressed with the evidence adduced for either of the other two.) There are also accounts of two cases of "death-bed visions," one of which (Lady Barrett's Case) seems to be about as well attested as such things can be. (For my own part I agree with Saltmarsh, who is quoted in the next chapter, in attaching very little weight to "death-bed visions.")

In the next two chapters the reader is presented with the main arguments against using apparitions as evidence for survival, and with rebuttals of these arguments. Here we must distinguish between (1) skepticism as to the *testimony* adduced for apparitions of the dead, and (2) doubts as to whether, granted that they occur, they are good evidence for the *survival* of the person whose apparition is seen. As regards (1), the criticisms of Dr. West, in his *Psychical Research Today,* are quoted. Professor Hart claims to have formulated, in his "Six Theories About Apparitions," a "reliable rating scale for measuring evidentiality" of such reports. He was left with 165 reported cases, all of which came above a certain assigned minimal level on this scale. Of these, 83 came above and 82 below the median for the admissible range of the scale. He found no significant differences, such as West had alleged to exist between these two sub-classes, in respect of the reported features of the cases. (Those of us who are not experts in statistical meth-

ods would do well to await the comments of those who are in regard to this scale and these applications of it.)

As regards (2), most of the experts quoted (Gurney, Tyrrell, Murphy, and L. E. Rhine) are agreed that the experience called "seeing an apparition" is in itself a subjective hallucination on the part of the experient, and not some kind of nonsensory perception by him of a trans-subjective object, which might be called the "astral body" of the person concerned. This conclusion has been reinforced by reference to (i) the *clothes* and other appurtenances of apparitions; and (ii) the occurrence of apparitions of (a) *animals* and (b) the *experient himself*. The views of these experts differ in minor details. All would agree that telepathy, at any rate from the *living,* plays an essential part in the generation of such experiences; and some (e.g., Tyrrell) would allow that in some cases telepathy from the surviving spirit of a dead person may do so. Mrs. Rhine and Gardner Murphy have stressed the part played by the emotional needs of the experient in generating the hallucination, in determining its details, and in assigning intention to the person referred to. They have insisted that in many cases of phantasms of the *living,* where the experient has ascribed intention to the person concerned, the latter had at the time no conscious intention of manifesting himself to the experient or to anyone else.

In dealing with these contentions Professor Hart lays great stress upon the following points: (1) In many cases where several persons have been together, they have all had *quasi*-perception as of *one and the same* external object (generally a human form resembling the physical body of a certain individual, whether dead or still alive) as it would appear if it were an actual physical object seen by them from their several points of view. This suggests the presence of an extra-subjective entity, localized in ordinary physical space, though Tyrrell has tried to deal with the facts on telepathic lines. (2) These must be taken along with the reported facts about what Professor Hart calls "ESP projection." Some of these cases are spontaneous, and some are experimentally produced either by hypnotic suggestion or by some process of mental concentration. The feature common to all of them is that the subject appears to himself to go out of his physical body (which he often "sees" as

from outside it), to travel to some distant place, and to perceive from that transferred point of view persons and things in its neighborhood. Often, though by no means always, the subject appears to himself to be provided with a nonphysical counterpart to his ordinary physical body, which is the center from which he temporarily perceives and acts. Occasionally, while A is having such an experience, B, to whose neighborhood A seems to himself to have been translated, has a *quasi*-perception as of A's bodily presence. Suppose (as happens in some of these cases) that A's report of what he ostensibly observed of B's actions and surroundings should prove to be correct, and suppose that B should simultaneously have had a *quasi*-perception as of A's bodily presence. Suppose, finally, that C and D, who were in B's company at the time, should have shared in this *quasi*-perception of B's. Then the interpretation of the facts in terms of an extra-subjective, extended and localized counterpart to A's physical body, which functions temporarily as the vehicle of A's consciousness, would be greatly strengthened; and the purely telepathic interpretation would have to be made so complex as to lose much of its plausibility.

Professor Hart's conclusion is roughly the following. We can most plausibly interpret apparitions of the *dead* by comparison with those apparitions of the living in which the person who was "seen" reports that he was conscious of being present in the neighborhood of the person or persons who "saw" him, and that he remembers "perceiving" the latter and their surroundings as from a counterpart of his physical body located among them. The conditions which are known to favor the occurrence of such apparitions of the *living* (e.g., a strong emotional link, intense concentration on the person to whom one wishes to appear, and so on) may reasonably be supposed to be involved in the case of apparitions of the *dead*. He realizes, however, that the phenomena of "haunting" do not fit at all well into this scheme. He is inclined to agree with Professor Price that, *if* the apparitions seen in a "haunt" are ever due to a surviving personality, the latter is probably quite unaware of what is happening and has no particular end in view.

I think that the author has rendered a valuable service to psychical research by his insistence on "out-of-the-body" cases in gen-

eral, and of collective and reciprocal apparitional cases in particular. Quite apart from psychical researchers, psychologists and those philosophers who concern themselves with the problems of sense perception would do well to make themselves acquainted with the reported facts.

The fifth and final Book consists of five chapters (XIII to XVII). One of these, Chapter XIV, deals with a topic which is quite fundamental but is somewhat isolated from the rest; viz., "Can Consciousness Survive the Destruction of the Brain?" I will comment on this first.

I think that the point at issue may be stated as follows. A human being in this life certainly has to be considered under two fundamentally different and mutually irreducible categories; viz., (1) as a *conscious and self-conscious being* who has experiences of various kinds, recognizes himself as the common owner of these experiences, and so on; and (2) as a living *physical organism*, with a highly complex brain and nervous system, in which specific electrical and chemical processes are continually going on. Now it is quite certain that there is an extremely close connection in this life between (a) the persistent structure of a man's brain and nervous system and the specific processes which are taking place in it at any time, and (b) the mental powers and dispositions which that man then manifests and the experiences which he then evinces or reports or which he later claims to remember having had at that time. The question is whether that relationship is such that the existence, the organization, and the functioning of a man's brain and nervous system are a *necessary condition* of his existing and functioning as a conscious and self-conscious being, with such-and-such a character, disposition, and experiences. If so, there can be no *possibility* of human survival of bodily death, and all the paranormal phenomena which would *prima facie* seem to point in that direction must be explained in other ways, no matter how far-fetched.

Professor Hart considers this question with special reference to recent experiments of Penfield on the results, in the way of reported experiences, of electrically stimulating certain areas of the brain. His conclusion is that the simplest hypothesis, in view of *all* the known facts about localization in the brain, is that "consciousness

uses the brain as its instrument—not only for perception and action, but also as a means of accumulating experience and for progressive development." I must confess that I do not find the argument for this conclusion clear or convincing, and that I doubt whether the metaphor of agent and instrument is particularly appropriate to the peculiar relationship between the mental and the cerebral aspects of a human being in this life. The reader must judge for himself.

The other four chapters of Book V are devoted to formulating and developing a certain theory, which Professor Hart calls the "Persona Theory." This, he thinks, enables one to synthesize the various aspects of the facts, both of mediumistic communications and of apparitions, some of which *prima facie* favor the hypothesis of survival, and others of which *prima facie* make against it.

I regret that I do not myself understand the Persona Theory, as a whole, well enough to venture to summarize it for others. As regards ostensible communicators through mediums, it seems to me not to differ essentially from the view developed by Mrs. Sidgwick in her study of Mrs. Piper's trance. This, as I understand it, may be stated as follows. What the sitter is *primarily* in touch with in all such cases is some stratum of the medium herself, unwittingly playing one dramatic role or another, just as what the play-goer is primarily in touch with is an actor playing now the part of one historical character, e.g., Henry V, now that of another, e.g., Cardinal Richelieu, and now that of a purely fictitious character, e.g., Hamlet as conceived by Shakespeare. Different parts played by the same actor have certain characteristic resemblances, and the same part played by different actors has characteristic differences. The same is true *mutatis mutandis* of the characters who present themselves at sittings as this or that person communicating through such and such a medium. This type of theory would leave room for purely fictitious pseudo-communicators, originating in the thoughts and intentions of this sitter, such as "Bessie Beals" and "John Ferguson," and for pseudo-communicators masquerading as the surviving spirits of persons who turn out to be in fact still alive, such as the chauffeur Réallier. But it would also allow us to hold, if we had specific evidence which seemed to require that explanation, that *some* of these dramatic poses derive their characteristic personal

traits and some of the characteristic bits of knowledge which they display from the immediate agency of the surviving spirit of the person represented; (c.f., e.g., the imaginary case of an actor, under the hypnotic influence of Mr. X, playing the part of Mr. X to the life).

This, however, is certainly not the whole of the Persona Theory, even if it be a correct interpretation of one part of it. For Professor Hart applies it to *apparitions*. He contends that some at least of these are "persons which have taken temporarily visible form, and which (in various degrees) may serve as vehicles through which a certain I-thinking consciousness can experience and act." He evidently considers the analogy of the "actors" and the "scenery" in ordinary *dreams* to be important here, and he lays particular stress in this connection on cases of *shared* dreams, three of which he quotes in Chapter XVI. He states that "the persona . . . . . consists in the entire structure" [of the individual who is "appearing"?] ". . . including his body, his clothing, his memories and purposes, and his ideas, attitudes, and interests." He alleges that the percipient's experience of a foreign persona may be "of a visual, auditory, or tactual character, or may be telepathic." But he adds that in some cases the persona of the individual who is appearing is "to a greater or less extent materialized" and that the percipient's experience of it "is *actually* visual or auditory or tactual, and that it may be shared by more than one *non*-clairvoyant percipient." I take this to mean that, in the latter cases, the awareness by the percipients of the apparition takes the form of *actual sense perception* while in the former it is some kind of *nonsensory* perception, clothed in appropriate visual, auditory, or tactual hallucination.

I must confess that I find all this very hard to follow. I cannot but think that there is a fundamental ambiguity in the phrase, quoted above, that a man's persona includes *inter alia* "his body and his clothing. . . ." This is true only if we interpret it to mean "includes his own *habitual ideas* of his body and of his clothing, the *feelings* which he continually gets from processes within his body and from its contacts with his clothing . . . ," and so on. But the notion of the apparitional body as a *vehicle* by means of which a person (whether temporarily in out-of-the-body cases, or perma-

nently after death) perceives foreign objects and acts upon them presupposes that it is an *extra-subjective* existent entity. What I do not understand is the connection between the purely *intra*-subjective (or at most *inter*-subjective) factor, in terms of which the notion of a persona has to be formulated, with the *extra*-subjective implications of Professor Hart's account of apparitions.

While I cannot but feel dissatisfied with the Persona Theory as stated, I certainly do not wish to end on a note of negative criticism. I would very strongly recommend the book as a whole, and I hope that the author will see his way to clarify the theoretical parts which I have found difficulty in grasping.

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PARAPSYCHOLOGICAL ABSTRACTS

SARA R. FEATHER

[This section consists of abstracts of reports published in other journals and of unpublished reports filed at the Parapsychology Laboratory at Duke University. Readers may obtain unabridged copies (Thermo-Fax) of the unpublished reports by writing to the *Journal of Parapsychology,* College Station, Durham, N. C. The price is ten cents per page.]

FITZHERBERT, J. The role of extra-sensory perception in early childhood. *J. Ment. Sci.,* 1960, **106**, 1560-7.

A free and universal occurrence of telepathy between mothers and their young children is hypothesized as a possible explanation for certain groups of puzzling phenomena. These include what appear to be memories of events which occurred in very early life, "inherited memories," instances of apparent innate knowledge, and the undoubted response of young children to the unconscious emotional attitudes of their mothers. If these speculations are true, they might further explain the occurrence of infantile amnesia, as well as the extreme importance for the child's satisfactory psychological development of the continuous presence of one single mother figure during the early years.

FRIEDMAN, S. M., AND FISHER, C. Further observations on primary modes of perception. *J. Amer. Psychoanal. Assoc.,* 1960, **8**, 100-29.

An experimental study was conducted to validate the observation that subliminal registration of a visual stimulus can occur even though the stimulus is brought below the subject's conscious threshold of perception by covering it with sheets of translucent paper. A checklist was used to quantify the degree of correspondence between images formed by the subjects as they viewed the apparently blank experimental stimulus and the actual target image, which was a simple line-drawing concealed underneath seven sheets of translucent paper. Subjects scored significantly higher after viewing this

experimental stimulus than after viewing a blank control stimulus which contained no concealed drawing, and this difference (CR = 2.33) is cited as evidence of subliminal visual stimulation.

In a later experiment in which subjects received a sub-hallucinatory dose of an "ego-impairing" drug (LSD-25), the scores for the experimental stimulus were higher than when no drug was used (CR of the difference = 2.47), which suggests that the drug does "increase the degree of delivery of percepts into subsequent imagery."

ESP is discussed, although rejected, as a possible alternative hypothesis to subliminal visual stimulation in the above experiments. The authors argue that if ESP were operating, the scores for the control images hidden in the same room with the subjects during presentation of the control stimulus should have been as high as those for the experimental image. Also, in a preliminary pilot study 10 sheets of translucent paper were used to mask the target, and the results differed from the seven-sheet procedure in a manner explainable by the subthreshold difference caused by the three sheets, whereas the writers think that ESP should have resulted in no such difference.

After briefly reviewing the parapsychological literature (Volumes 1, 17, and 18 of the *Journal of Parapsychology*), the authors note that some, but not all, of the results claimed for parapsychology in these volumes may be based instead upon subliminal registration.

MURPHY, G. Experimental and other methods in parapsychology. *Indian J. Parapsychol.*, 1960, **2**, 41-47.

When the role of the experiment in parapsychology within recent years is examined, it appears that experimental method has not really captured the central issues responsible for the positive results. The conclusion Murphy reaches is, not that this method be given up, but that effort be directed to a certain kind of experiment. This would be experimental work based upon careful study of the existing evidence and a careful definition of a hypothesis to be tested, which then "absolutely requires" a careful experimental plan and a statistical method of evaluating the outcome.

MURPHY, G. Future research in precognition. *Int. J. Parapsychol.*, 1960, 2, 5-20.

In this paper embodying the summary remarks which the author made as chairman of a conference on precognition (New York, 1959), the broad field of precognition is briefly considered and an attempt is made to bring together major strands of thought on the subject.

Preliminary to future research in precognition, Murphy feels, should come a critical evaluation of the literature and perhaps a redefinition of the phenomena so as to place precognition in its broader perspective, perhaps among the life sciences in general. Efforts should then be made to classify the phenomena along different dimensions to see which will lead to new discoveries as they are put to the test. Methods of study should involve analysis of life history material, first from single gifted individuals, then from small groups, and finally from larger groups, as is now more customarily done. The experimenter might even make clinical observations concerning his own experiences. Future experiments will incorporate proposals from many directions, such as the phenomenological and depth approaches, from situational analysis, and from the taxonomic studies.

Murphy feels that a conceptual model for precognition would be helpful even at this stage, and in order to clarify this concept he outlines one particular model, Whately Carington's association theory, which he describes as "a fantastically well thought out and ingenious theory."

NASH, C. B. A comparison of two agents in GESP and clairvoyance tests. *Indian J. Parapsychol.*, 1960, 2, 60-64.

When the GESP and "down through" clairvoyance scores of 14 subjects were compared according to which of two brothers was acting as agent, the clairvoyance scores obtained when one brother was card-handler were found to be statistically significant ($P = .0026$) although the other groups of scores and the total deviation of the experiment were not of a significant order. A marginally significant chi-square ($P = .022$) was found when ESP hits and misses were analyzed according to the subject's attitude to ESP

("believer," "conflict," and "disbeliever") as determined by their ratings on the Van de Castle and White ESP attitude scale. No relation was found between ESP scores and personality interests as measured by the Allport-Vernon Study of Values.

PRICE, H. H. Paranormal cognition and symbolism. *Light,* 1960, 80, 53-60.

The assumption is made that below the level of consciousness we are constantly in touch parapsychically with a much wider range of facts and happenings than conscious awareness would indicate, but that some barriers or censorship prevent the flow of this psi information from coming into consciousness and cause much that does appear to take an inaccurate or distorted form. The use of the term "symbolism" to describe the various disguises or substitutions which psi may take in consciousness is considered unwise, however, because this term is "so very elastic that some of the assertions in which it is used can neither be decisively confirmed nor decisively refuted."

The important task for workers in this field is to find reliable methods of penetrating these disguises and substitutions in order to allow the parapsychically acquired idea to get through to consciousness. Possible ways of achieving this are discussed, including a "free association" method similar to one used informally by a group of Oxford students in betting on horse racing, and a method similar to that used by a creative writer in attempting to get "inspiration." More attention might be given to the role of the "first thought" in possible psi communication, because this may be one way in which psi information slips in from the unconscious and evades the Censor.

SCHMEIDLER, G. *ESP in Relation to Rorschach Test Evaluation.* Parapsychological Monographs, No. 2. New York: Parapsychology Foundation, 1960.

Subjects who took part in ESP tests primarily designed to study the effect of attitudes toward the experiment were also given the Rorschach test. With regard to the primary objective, the effect of attitudes, subjects who accepted the possibility of ESP (sheep)

scored significantly above mean chance expectation, while those who rejected it (goats) scored significantly below.

A preliminary group of Rorschach results suggested that the well-adjusted sheep and goats were chiefly responsible for the significant ESP scoring, while the poorly-adjusted subjects were grouped together around the chance mean. Further Rorschach results from more than 1,000 subjects were analyzed to provide a test of this suggestion. The Rorschach responses were always scored (using the Monroe method for quick evaluation by inspection) before the ESP results were known. The findings support the hypothesis that good adjustment is associated with the more significant ESP scoring in both the sheep and the goats.

There was a marked break in the ESP score level in both attitude groups at precisely that point in the Rorschach rating scale which had been selected as separating subjects with good and poor adjustment. The ways in which this discontinuity in the ESP scores might have arisen are considered. A re-examination of the Rorschach records suggests that the experimenter tended to place subjects who were on the borderline in the well-adjusted group if their responses showed liveliness and in the poorly-adjusted group if they demonstrated constraint.

A parallel study of the same data was conducted in an effort to relate the ESP scoring more specifically to personality traits as revealed by the Rorschach responses. In a preliminary group of 250 subjects, seven criteria or "signs" in the Rorschach responses were selected which seemed to be associated with failure to demonstrate ESP. The effectiveness of using the presence or absence of these signs for separating unsuccessful and successful ESP results was then tested in the remaining 754 subjects. A significant separation in the direction of the prediction was achieved, and the possible psychological significance of each of the separate Rorschach signs is discussed.

The general conclusion, "stated hesitantly," is that feelings of constraint, withdrawal, or negativism are associated with chance results or below-chance ESP scoring, while feelings of free responsiveness are associated with successful (positive) ESP scoring.

—*J. G. Pratt*

SCOTT, C. An appendix to "The Repeated Guessing Technique." *Int. J. Parapsychol.*, 1960, 2, 37-46.

A statistical method is presented for estimating (under certain restrictions) how much concentration would be expected to occur for a given number of kinds of targets and for various levels of correct guessing. This method shows, for example, that with five kinds of targets and an average scoring rate of seven per pack of twenty-five cards, the expected number of runs necessary for a concentrated score of 80% would be 70, while 150 runs at that level of scoring would bring the concentrated score up to 95% correct.

This method can also be used to determine the exact relation between the degree of concentration and the ensuing loss of significance as compared with the original data. As a general rule, the critical ratio for majority-vote scoring will be between 70% and 80% of the binomial critical ratio.

SOMMER, R., OSMOND, H. AND PANCYR, L. Problems of recognition and identity. *Int. J. Parapsychol.*, 1960, 2, 99-123.

Little is definitely known about how personal identity is established in our society, and this remains a crucial research problem for psychiatrists and psychologists, as well as a basic question for parapsychologists interested in the survival question. Pilot studies showed that the type of statements made by individuals to prove their identity varied according to the closeness of relationship between the people. The extent to which these statements were then convincing to respondents in identifying these persons was found to depend to a significant extent upon whether or not the strong likelihood of fraud or imposture had been suggested to the respondent. When a third person or "filter" was used in transmitting the identifying material, a significant amount of distortion was noted. Some of the difficulties found in communications of identity are discussed, and a number of suggestions considered worthy of future investigation are made.

THOULESS, R. H. The repeated guessing technique. *Int. J. Parapsychol.*, 1960, 2, 21-36.

A repeated guessing technique (RGT) is described whereby repeated guesses at a particular target card are treated as "votes"

for the symbol on the card, so that when ESP is operating an increasing number of target cards will come to be correctly designated as the number of repetitions of the guessing process is increased. The technique is not a means for enhancing the significance of ESP data, but it is proposed as a means of getting a higher rate of correct or exact information out of the data. Thouless suggests that, if certain conditions are fulfilled, the accuracy of description with the RGT might increase near enough to 100% to be sufficient for effective communication.

Some difficulties encountered in the initial application of the RGT are described and suggestions are made for future experimentation with it. The "concentration" possible could be limited, for instance, if the excess right guessing is not distributed to some extent over all the positions and over all the symbols in the pack. The difficulty[1] can be overcome by using a method in which a series of target cards is presented in varying order while each card retains its identity (such as a deck of playing cards or a deck of individualized ESP cards). No obvious way has been found for overcoming the difficulty arising from excess right guessing being restricted to certain symbols, but this may not be a real difficulty. Examination of the three sets of data in which such an analysis was possible (Soal-Goldney, Warner, and Pearce-Pratt experiments) showed that the excess right guesses were distributed over all symbols.

Two methods of calculating from the guesses which card is the most favored are described, a simple majority-vote method and an "index of preference" which gives the ratio between the number of times each symbol is actually guessed for a particular target card and the number of times the symbol is expected to be guessed by chance. The application of both methods to actual data is described, and the practical aspects of their use are discussed.

[1] From possible differential scoring rates on different positions.

THE PARAPSYCHOLOGICAL ASSOCIATION

Fourth Annual Convention

The fourth annual convention of the Parapsychological Association will be held in New York City, September 7-9, 1961, at the Henry Hudson Hotel. Registration of members and visitors will begin at 9:30 A.M. on Thursday, September 7, and Dr. Karlis Osis, President of the Association, will call the convention to order at 10:30. The presidential address will be given on Thursday afternoon. At the convention dinner, on Friday evening, Dr. Michael Scriven will give an invited address on the topic: "New Frontiers of the Brain."

Other sessions are planned at which research papers will be presented under the general headings of stimulus variables in ESP, subject variables in ESP, précognition, and novel problems and experimental techniques. The program will also include sessions in which panels will lead discussions on ESP in the schoolroom, the problem of post-mortem survival, and possibly other general topics as well.

A special session will be devoted to brief reports on work planned or in progress. Anyone desiring to present a "research brief" should write to Dr. J. G. Pratt, Parapsychology Laboratory, Duke University, Durham, N. C., not later than the end of July.

The purpose of this announcement is to inform the membership of the Association, and others who might wish to attend the convention, about the general arrangements. Members and Associates will receive a copy of the full program and other useful information regarding hotel reservations, etc., prior to the convention date. It is hoped that those who will be seeking hotel accommodations will plan to stay at the Henry Hudson, where special rates have been offered by the management.

The chairman of the Program Committee is Dr. Gertrude Schmeidler (17 Kent Ave., Hastings-on-Hudson, N.Y.), and convention arrangements are being handled by Miss Rhea White (77 Pilgrim Lane, Westbury, L. I., N.Y.).

CORRECTION

The analysis presented in the table on p. 164 of the *Journal* for September, 1960, should be interpreted as indicating that those subjects who rated themselves as more lucky had also a larger number of PK game scores above "chance" than did subjects who rated themselves as less lucky. The data used in this analysis are the game scores of eight subjects who received a luck questionnaire, and the impression is erroneously given by the article that the analysis indicated a correlation between subjects' luck ratings and their PK performance. Such a correlation would require that the *total* PK results of each subject should be taken as the statistical unit for comparison with his luck rating, and eight subjects would not be a sufficient number for any conclusion. The analysis does show that the luck questionnaire was used in this case to separate the more successful PK test results from the less successful ones, but gives no ground for concluding that there is any causal connection between luck rating and PK success. However, the trend shown in the analysis is consistent with the existence of such a causal connection, and the results offer encouragement for the further investigation of PK performance and luck ratings on a more extensive basis.

GLOSSARY

A description of the basic experimental methods, of the findings, and of the statistical procedures for evaluating ESP and PK results may be found in *Parapsychology, Frontier Science of the Mind* by J. B. Rhine and J. G. Pratt (published by Charles C. Thomas, Springfield, Illinois, U.S.A., Blackwell Scientific Publications, Ltd., Oxford, England, and The Ryerson Press, Toronto, Canada).

AGENT: The "sender" in tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

BT (Basic Technique): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

Chance Expectation = Mean Chance Expectation: The most likely score if only chance variation is present.

Chance Average: Mean chance expectation expresed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic cognition of the mental activities of another person.

CR (Critical Ratio): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearsons.)

CR_d (Critical Ratio of the Difference): The observed difference between the the average scores of two samples of data divided by the standard deviation of the difference.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

Backward Displacement: ESP responses to targets preceding the intended targets. Displacement to the targets one, two, three, etc. places preceding the intended target are designated as (−1), (−2), (−3), etc.

Forward Displacement: ESP responses to targets coming later than the intended targets. Displacement to the targets one, two, three, etc. places after the intended target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the pack before any are removed or checked.

ESP (Extrasensory Perception): Awareness of or response to an external event or influence not apprehended by sensory means.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, square, cross and waves (three parallel wavy lines).

ESP PACK: Twenty-five ESP cards.

Closed Pack: An ESP pack composed of five each of the five symbols.

Open Pack: An ESP pack made up of the ESP symbols selected in random order.

EXPECTATION: See *Chance.*

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects in employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

GESP (General Extrasensory Perception): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

MCE (Mean Chance Expectation): see *Chance.*

P (Probability): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARANORMAL: Parapsychical or parapsychological.

PARAPSYCHOLOGY: A division of psychology dealing with behavioral or personal effects which do not fall within the scope of physical principles.

PERCIPIENT: See *Subject.*

PK (Psychokinesis): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PLACEMENT TEST: A PK technique in which the aim of the subject is to try to influence falling objects to come to rest in a designated part of the throwing surface.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring responses to free material. A judge ranks the stimulus objects (usually pictures in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes which are non-physical in nature. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI-MISSING: Exercise of psi ability in a way that avoids the target the subject is attempting to hit.

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

PSYCHICAL RESEARCH: Older term used for parapsychology.

QD (Quarter Distribution): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RANDOM ORDER: An order of events which displays no trends or regularities that would allow any inference regarding one event from one or more of the others in the series.

RUN: A group of trials, usually the succesive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in any given unit of trials.
Total Score: Total of scores divided by number of runs.
Average Score: Total score divided by number of runs.

SD (Standard Deviation): Usually the theoretical root means square of the deviations. It is obtained from the formula $\sqrt{npq}$ in which n is the number of single trials, p the probability of success per trial, and q the probability of failure.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in parapsychology today is a probability value of .01 or less, or a deviation in either direction such that the CR is 2.58 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

SPONTANEOUS PSI EXPERIENCE: Natural, unplanned, occurrence of an event or experience that seems to involve parapsychical ability.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

STM (Screened Touch Matching): An ESP card test in which the subject indicates in each trial (by pointing to one of five key positions) what he thinks the top card is in the inverted pack held by the experimenter behind a screen. The card is then laid opposite that position.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP (including precognition) the objective or mental events the subject is attempting to respond to. In PK tests, the faces of the die which the subject attempts to bring up in the act of throwing. In placement tests, the designated part of the throwing surface.

Target Card: The card which the percipient is attempting to identify or otherwise indicate a knowledge of.

Target Pack: The pack of cards the order of which the subject is attempting to identify.

Target Face: The face on the die which the subject tries to turn up by PK.

TELEPATHY: Extrasensory perception of the mental activities of another person (as distinguished from clairvoyant cognition of objective events).

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.